



The impact of upzoning on housing construction in Auckland[☆]

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ABSTRACT

There is a growing debate about whether upzoning is an effective policy response to housing shortages and unaffordable housing. This paper provides empirical evidence to further inform debate by examining the various impacts of recently implemented zoning reforms on housing construction in Auckland, the largest metropolitan area in New Zealand. In 2016, the city upzoned approximately three quarters of its residential land to facilitate construction of more intensive housing. We use a quasi-experimental approach to analyze the short-run impacts of the reform on construction, allowing for potential shifts in construction from non-upzoned to upzoned areas (displacement effects) that would, if unaccounted for, lead to an overestimation of treatment effects. We find strong evidence that upzoning stimulated construction. Treatment effects remain statistically significant even under implausibly large displacement effects that would necessitate more than a four-fold increase in the trend rate of construction in control areas under the counterfactual of no-upzoning. Our findings support the argument that upzoning can stimulate housing supply and suggest that further work to identify factors that mediate the efficacy of upzoning in achieving wider objectives of the policy would assist policymakers in the design of zoning reforms in the future.

1. Introduction

Housing has become prohibitively expensive in many of the world's major cities, precipitating serious and widespread housing affordability crises (Wetzstein, 2017). A growing coalition of researchers argue that part of the solution is to “upzone” cities by relaxing land use regulations (LURs) to allow construction of more intensive housing, such as townhouses, terrace housing and apartment buildings (Glaeser and Gyourko, 2003; Freeman and Schuetz, 2017; Manville et al., 2019). Policymakers have begun to listen to these supply-side solutions and, in response, several metropolitan and gubernatorial authorities have pursued zoning reform in recent years (National Public Radio, 2019).

These policy reforms are underpinned by the argument that LURs increase house prices by restricting housing supply (Glaeser et al., 2005; Quigley and Raphael, 2005; Quigley and Rosenthal, 2005; Ihlanfeldt, 2007; Zabel and Dalton, 2011; Gyourko and Molloy, 2015). Relaxing those regulations through upzoning, it is argued, enables new more intensive development, thereby increasing housing supply and putting

downward pressure on prices. However, these arguments are not universally accepted and many commentators remain skeptical of the capacity for these market-led policies to deliver affordable and inclusive housing (Rodríguez-Pose and Storper, 2020; Wetzstein, 2021). Instead it is suggested that government intervention is needed to tackle the problem through means such as state-led construction (Wetzstein, 2021; Favilukis et al., 2023), the repurposing of public space (Wetzstein, 2021; Freemark, 2021), and policies that limit demand (Wetzstein, 2021).

Unfortunately our understanding of the manifold impact of upzoning is presently limited by an acute lack of empirical research on the subject (Schill, 2005; Freeman and Schuetz, 2017; Freemark, 2019). Only a handful of studies have offered empirical evidence of the effects of a relaxation of LURs (Atkinson-Palombo, 2010; Freemark, 2019; Gray and Millsap, 2020; Limb and Murray, 2022; Peng, 2023), and these have tended to concentrate on small-scale policy changes, often involving transit-oriented rezoning, not the large-scale policy reforms currently being implemented in many US cities.¹ The limited empirical work that is available has findings that often contravene the

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¹ Gray and Millsap (2020) is an exception, showing that the 1998 reduction in minimum lot sizes in Houston preceded an increased concentration of development activity in middle-income, less dense, under-built neighborhoods.

outcomes anticipated by proponents of supply-side regulatory reforms. For example, [Freemark \(2019\)](#) found that transit-oriented upzoning in Chicago failed to encourage construction, calling into question the fundamental premise of the supply-side argument ([Rodríguez-Pose and Storper, 2020](#)). Meanwhile, [Limb and Murray \(2022\)](#) argue that transit-oriented upzoning in Brisbane, Australia, generated no significant increase in housing construction.

The lack of empirical evidence on the effects of large-scale upzoning is largely due to the fact that, until very recently, no city has systematically upzoned large shares of land as a mechanism to promote affordability ([Freeman and Schuetz, 2017](#), p. 229). In 2016, however, the city of Auckland, New Zealand, implemented large-scale zoning reforms under the Auckland Unitary Plan (AUP). Motivated in part by housing affordability concerns ([Auckland Unitary Plan Independent Hearings Panel, 2016](#)), the plan upzoned approximately three-quarters of residential land, and trebled the number of dwellings that could be built ([Greenaway-McGrevy et al., 2021](#)), providing researchers with a unique opportunity to study large-scale upzoning reform of the kind that was hitherto lacking.

The present paper examines impact of upzoning on housing construction in Auckland to provide further evidence on whether zoning reforms can fulfill the fundamental premise of the supply-side policy response, namely, that upzoning increases housing supply. A difference-in-differences (DID) framework is adopted that exploits geographic variation in the incidence of upzoning to estimate causal effects through the comparison of outcomes in upzoned residential areas with outcomes in non-upzoned residential areas. Our dataset consists of geocoded building permits that are matched to planning maps that detail the geographic incidence and intensity of upzoning.² Because the empirical design exploits temporal changes in zoning rules via a policy intervention, it has the capacity to mitigate many of the concerns stemming from the endogeneity of regulations that afflict studies which rely only on spatial variation in LURs ([Gyourko and Molloy, 2015](#)). By exploiting time series and spatial variation in LURs, we are able to allow for a wider set of time-invariant confounders since their impact on outcomes is differenced out in the DID procedure. The approach is concordant with other work where changes in the geographic variation in zoning has been used in quasi-experimental designs to examine casual impacts ([Thorson, 1997](#); [Cunningham, 2006](#); [Zhou et al., 2008](#); [Kahn et al., 2010](#); [Freemark, 2019](#)).

Our empirical strategy pays particular attention to the possibility that housing construction in upzoned areas displaced housing that would have otherwise been constructed in non-upzoned areas.³ These displacement effects would manifest as negative spillovers from treatment areas to control areas, violating the stable unit treatment value assumption (SUTVA) in the Neyman-Rubin causal framework, and generating an overstatement of conventional treatment effects that are based on simple comparisons of outcomes in treatment and control groups. Present techniques for addressing spatial spillover effects require the spillovers to be localized, in the sense that the magnitude of the spillover between geographically distant areas is assumed to be negligible ([Clarke, 2017](#); [Butts, 2021](#); [Huber and Steinmayr, 2021](#)). As we demonstrate below, the evidence suggests that upzoning in Auckland reallocated permits over large distances (from distant areas with more vacant land to near areas with less vacant land), meaning that methods that model spillovers under the assumption that they are highly localized and dissipate with distance are untenable in our setting. We therefore develop an approach to accommodating spillover effects that does not rely on spillovers dissipating with distance.

² Throughout the paper, “permits” refers to permitted housing units or, in the New Zealand parlance, “consented dwellings”.

³ Spatial displacement effects go by varying terms in the urban literature. [Neumark and Simpson \(2015\)](#) characterize displacement effects from place-based policies as negative spillovers, while [Redding and Turner \(2015\)](#) refer to “reorganization” effects in response to transport policies.

We adapt the set identification approach suggested by [Rambachan and Roth \(2023\)](#) (hereafter “RR”) for remediating violations of the standard parallel trends assumption that is required under the DID framework. RR extrapolate pre-treatment trends to generate a set of counterfactual outcomes in the treatment group. In the present paper we repurpose this strategy by using pre-treatment trends in the *control* group to extrapolate a set of counterfactual outcomes that are used to bound the magnitude of the spillover effect. The intuition underpinning both the RR strategy and our strategy is the idea that observed trends immediately prior to the policy intervention are informative of the counterfactual scenario. Adapting the RR method to our application yields a confidence set of treatment effects that is robust to spillover effects and amenable to inference.

The empirical findings using this methodology reveal strong statistical evidence that upzoning increased housing construction. Our preferred model specification shows a statistically significant increase in permits even under counterfactual sets that span approximately four times the extrapolated linear trend in the control group. For example, a linear trend fitted to pre-treatment observations in the control group implies that 1116 additional permits in non-upzoned areas in 2021 under the counterfactual of no upzoning. We find that the estimated treatment effect for 2021 remains statistically significant even when we allow an additional 4469 permits in the counterfactual set of outcomes. Put differently, counterfactual scenarios that allow more than a four-fold increase in permits over the pre-treatment trend (since $4 \approx 4.005 = 4469/1116$) would be needed in order for the estimated treatment effects to become statistically insignificant. There is no policy change concurrent to the upzoning policy that could plausibly generate such a substantial increase in construction.

We also use the extrapolated counterfactual trend in control areas to generate a point estimate of the number of additional permits enabled through upzoning. To do so, we restrict the counterfactual set to the extrapolated linear trend, so that the set collapses to a point. This approach implies that 21,808 additional dwellings were permitted over the five years following the zoning reform, corresponding to approximately 4.11% of the dwelling stock of the Auckland region.⁴

Our results have implications for ongoing debates about the efficacy of upzoning. In particular, the findings support the view that large-scale upzoning can encourage construction. This is particularly important in the light of recent work by [Freemark \(2019\)](#) and [Limb and Murray \(2022\)](#), who find that zoning changes had minimal impact on housing construction in Chicago and Brisbane, respectively. Further work examining potential mediating factors that enabled increased construction will hopefully help explain why the policy was more effective in Auckland, and assist policymakers in tailoring rezoning and housing policies to facilitate construction elsewhere.

The remainder of the paper is organized as follows. Section two provides the background institutional context and timeline of the key events in the city of Auckland and Section three describes the dataset used in our empirical work. Section four presents the empirical DID model. Section five describes and applies our methodology for dealing with potential spillover effects. Section six concludes.

2. Institutional background

This section provides some background demographic and administrative features of Auckland city with information concerning relevant policies and processes preceding the relaxation of Land Use Regulations (LURs) under the Auckland Unitary Plan (AUP). It also shows how the policy informs our empirical design.

⁴ Note this is not an estimated increase in dwelling stock. Unfortunately we do not have precise measures of dwellings demolished when properties are redeveloped because demolition permits are only required for buildings that are less than three storeys.

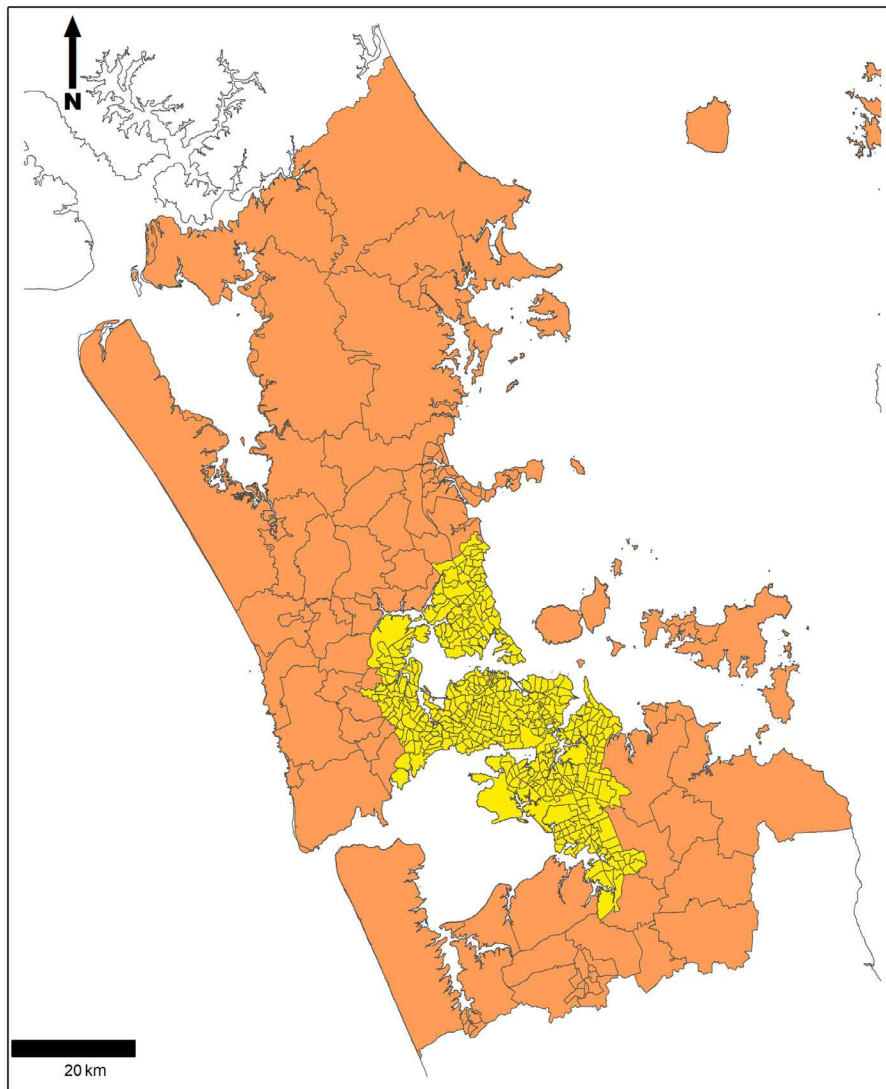


Fig. 1. Auckland region. *Notes:* Auckland region (shaded) decomposed into Statistical Area geographic units. Urban core shaded yellow in the center. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)

Auckland is the largest city in New Zealand with a population of approximately 1.57 million within the greater metropolitan region (as of the 2018 census). Prior to 2010, the metropolitan region comprised seven different city and district councils. Since 2010, the entire metropolitan area, as well as several towns, populated islands, and a large amount of the rural land beyond the fringes of its outermost suburbs, has been under the jurisdiction of a single local government, the Auckland Council. Centered on a long isthmus of land between two harbors, this jurisdiction extends over 4894 km² of land area.

Figure 1 illustrates the Auckland region, decomposed into Statistical Areas (SAs), which, as discussed below, are used as the geographic unit of analysis in our work. The shaded areas are the Auckland Council region. The lighter shaded area in the center is what we refer to as the “urban core”.⁵

In March 2013, the Auckland Council announced the “draft” version of the AUP. The draft version of the plan went through several rounds of consultations, reviews and revisions before the final version became operational on 15 November 2016. Each version of the AUP contained new LURs that would potentially change restrictions on the extent of site development. In most areas these regulations were relaxed in order to

enable residential intensification and greater population density, including multi-family housing such as terrace housing and apartments. These proposed changes could be viewed online, so that any interested member of the public could observe the specific LURs proposed for a given parcel of land. This meant that it was relatively simple for developers to observe the new land use regulations and to commence planning prior to the policy becoming operational.

The amount of development allowed on a given site is restricted by the LURs of its assigned planning zone. In this study we focus on four residential zones introduced under the AUP, listed in declining levels of permissible site development: Terrace Housing and Apartments (THA); Mixed Housing Urban (MHU); Mixed Housing Suburban (MHS); and Single House (SH). Thus THA allows the most site development, and SH allows the least. Table 1 summarizes the various LURs for each of the four residential zones considered. These regulations include site coverage ratios, height restrictions, setbacks and building envelopes, among others. For example, five storeys and a maximum site coverage of 50% is allowed in THA, whereas only two storeys and 35% site coverage is allowed in SH.⁶

⁵ For the urban core, we use statistical areas inside or overlapping the “Major Urban Area” of the greater metropolitan region, as defined by Statistics New Zealand.

⁶ There are two additional zones in the AUP that are classified as “Residential”: “Large Lot” and “Rural and Coastal Settlement”. We exclude these areas from our analysis as they are an intermediate, semi-rural zone between outright rural and urban housing areas. We also omit residential land on the islands in the Hauraki Gulf, which have their own unique zoning under the AUP.

Table 1
Summary of land use regulations by residential zone under the Auckland unitary plan.

Regulation	Terrace housing and apartments zone	Mixed housing urban zone	Mixed housing suburban zone	Single house zone
Max. height	16 m (five to seven storeys)	11 to 12 m (three storeys)	8 to 9 m (two storeys)	8 to 9 m (two storeys)
Height in relation to boundary	3 m vertical + 45° recession plane	3 m vertical + 45° recession plane	2.5 m vertical + 45° recession plane	2.5 m vertical + 45° recession plane
Setback	0 m	1 m	1 m	1 m
Site Coverage	50%	45%	40%	35%
Impervious Area	70%	60%	60%	60%
Min. dwelling size (1 bedroom)	45 m ²	45 m ²	45 m ²	n/a
Max. dwellings (on existing parcels)	does not apply	3	3	1
Min. Lot Size (subdivision)	1200 m ²	300 m ²	400 m ²	600 m ²

Notes: Tabulated restrictions are “as of right” and can be exceeded through resource consent notification. Number of storeys (in parentheses) are obtained from the stated purpose of the height restriction in the regulations. Height in relation to boundary and setbacks apply to side and rear boundaries. Less restrictive height in relation to boundary rules than those tabulated apply to side and rear boundaries within 20m of site frontage. Maximum dwellings per site are the number allowed as of right. Minimum lot sizes per dwelling do not apply to existing residential parcels. Site coverage is the area under the dwelling structure. Impervious area is the area under the dwelling and structures such as concrete driveways that prevent rainwater absorption into soil.

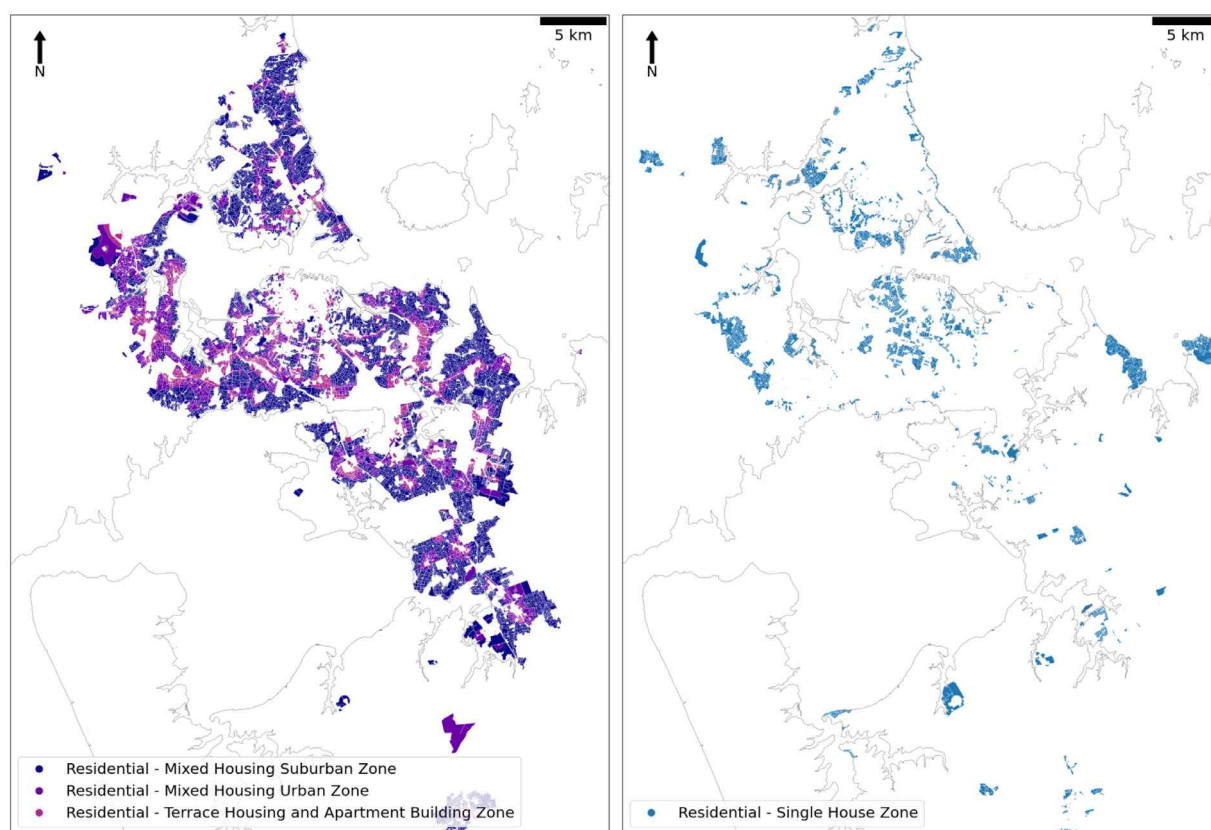


Fig. 2. Residential Zones of inner Auckland introduced under the Auckland Unitary Plan.

Figure 2 depicts the geographic distribution of the four zones. For clarity we zoom in on the central urban area of Auckland. Evidently MHS covers the most area, closely followed by MHU. SH is predominantly located either very close to the central business district (CBD) or at the outskirts of the city. THA covers the least amount of area. Parcels zoned as THA, MHU or MHS collectively comprise 75.1% of residential land, while SH accounts for the remaining 24.9%. Within the urban core of Auckland (the central area in Fig. 1), THA, MHU and MHS account for 84.3% of residential land.⁷

⁷ Areas calculated using geocoded dataset of cadastral land parcels, defined as at November 2016.

Our empirical design uses the introduction of the AUP as a quasi-natural experiment in which residential areas that were upzoned to either MHS, MHU or THA are designated as treatment areas, while residential areas that were not upzoned (including SH) are control areas. In order to identify upzoned areas, we compare the LURs that applied before and after the AUP. Prior to the AUP, each of the seven city and district councils set their own zoning regulations, which remained in effect until the AUP became operational in 2016. Upzoned areas are identified by comparing the maximum floor area ratios (FARs) under the previous zone to those that applied under the zone introduced under the AUP. Areas where the FAR increased are classified as upzoned. Although neither the AUP nor the seven city and district plans placed direct restrictions on FARs, all sets of plans imposed site coverage ratios and height limits

that cap the maximum allowable FAR on a parcel. Under a plausible mapping of building height to storeys, the FAR is straightforwardly the product of the storey limit and the site coverage ratio. Further details on the classification method are given in the Appendix.⁸

Using this approach, approximately three-quarters (75.2%) of all residential land (SH, MHS, MHU and THA zones combined) is classified as upzoned, including the vast majority (98.7%) of the land designated as either THA, MHU or MHS.⁹ Meanwhile, 96.2% of residential land had a FAR no greater than that of SH under the AUP. Thus, while the previous set of regulations did allow for housing intensity that was roughly equivalent to MHS, MHU or THA, these zones were restricted to very small, targeted areas.

Our sample period spans several demand-side policies intended to curb housing demand to promote affordability, including severe restrictions on foreign ownership, disincentives to investor speculation, and macroprudential banking restrictions.¹⁰ Although these policies frequently exempted new builds, there is little reason to think that these policies would differentially impact upzoned areas. Moreover, as we illustrate below, the parallel trends assumption holds remarkably well in our data, affirming that these policies did not have a differential impact. On the supply side, prior to the AUP, “Special Housing Areas” (SpHA) incentivized developers to provide a limited amount of affordable housing in exchange for accelerated processing of building permits. Developers could also use more relaxed planning rules from a preliminary version of the plan (the “Proposed AUP”, notified in September 2013). SpHAs were disestablished once the AUP became operational. We exclude permits issued in SpHAs prior to 2017 as a disproportionate share of SpHA permits are in locations that were later upzoned. A robustness check reported in the Appendix demonstrates that our findings are largely unaffected when these permits are included in the analysis.

3. Dataset

Our dataset is based on annual building permits for new dwelling units issued by the Auckland Council from 2010 to 2021.¹¹ The permits include the number of dwellings. Each observation includes the longitude and latitude of the building site, which have been used to map each permit to its corresponding zone under the Auckland Unitary Plan (AUP).

Figure 3 exhibits aggregate permits in upzoned and non-upzoned residential areas over the 2010 to 2021 period. We also decompose permits into attached and detached dwellings. There is a clear increase in the number of permits in upzoned areas after the policy is implemented (from 2017 onward). The number of attached dwelling permits per year in upzoned areas increases from under 1000 in 2016 to near 10,000 by 2021 – more than a tenfold increase. Over the same period, detached housing increases from just over 2000 permits per year to approximately

⁸ FARs are often used as a measure of LUR stringency; see Brueckner et al. (2017); Brueckner and Singh (2020) and Tan et al. (2020). By using a FAR, our upzoning identification algorithm is based on increases in maximum floorspace density, rather than restrictions on dwelling density, such as minimum lot sizes (MLS). While the majority of the zones prior to the AUP also had MLS in addition to height and site coverage restrictions, MLS do not apply to extant residential parcels under the AUP. MLS on extant parcels were therefore an additional restriction that only applied prior to the AUP, which means that our upzoning classification method may understate the amount of upzoned land.

⁹ We also consider empirical designs in which THA, MHU and MHS are simply classified as the treatment group and SH areas are the control group, and where downzoned areas are excluded from the analysis. Our results and findings do not substantively change.

¹⁰ Loan to value ratios on new residential mortgages were introduced in 2013; a limited capital gains tax was introduced in 2015; and legislation preventing foreign ownership (excepting Australia and Singapore) in 2018. See Greenaway-McGrevy and Phillips (2021) for additional details.

¹¹ Permits for extensions to existing dwellings are not included in our analysis.

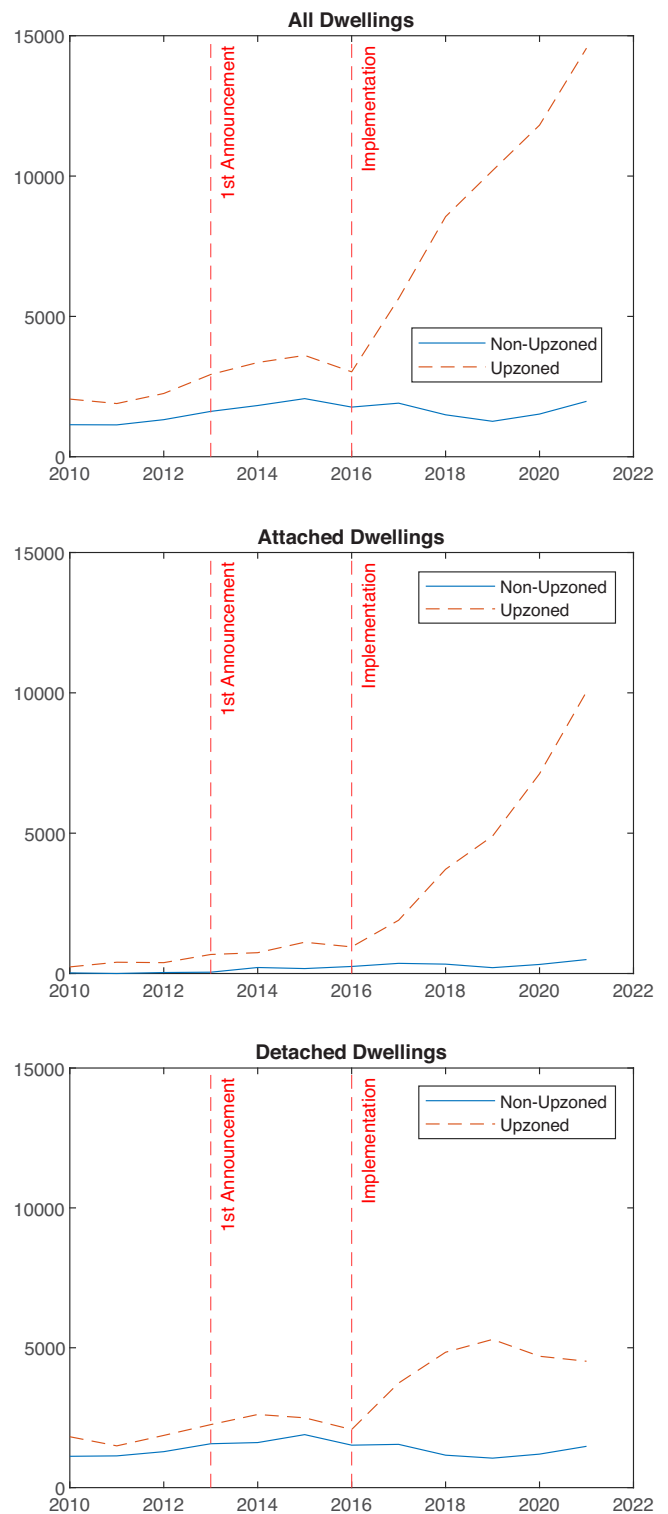


Fig. 3. Dwelling permits, 2010–2021. Notes: Dwelling permits issued in upzoned and non-upzoned residential areas.

4500. By 2019, there were more attached dwelling permits than detached, consistent with the upzoning goal of incentivising more capital intensive structures. In addition, there is a notable fall in detached dwelling permits between 2019 and 2021 in upzoned areas.

Prior to the policy change, permits in subsequently upzoned areas consistently exceeded permits in non-upzoned areas by a relatively constant amount. This pattern is consistent with modeling permits in levels in a difference-in-differences framework, since it implies an absolute

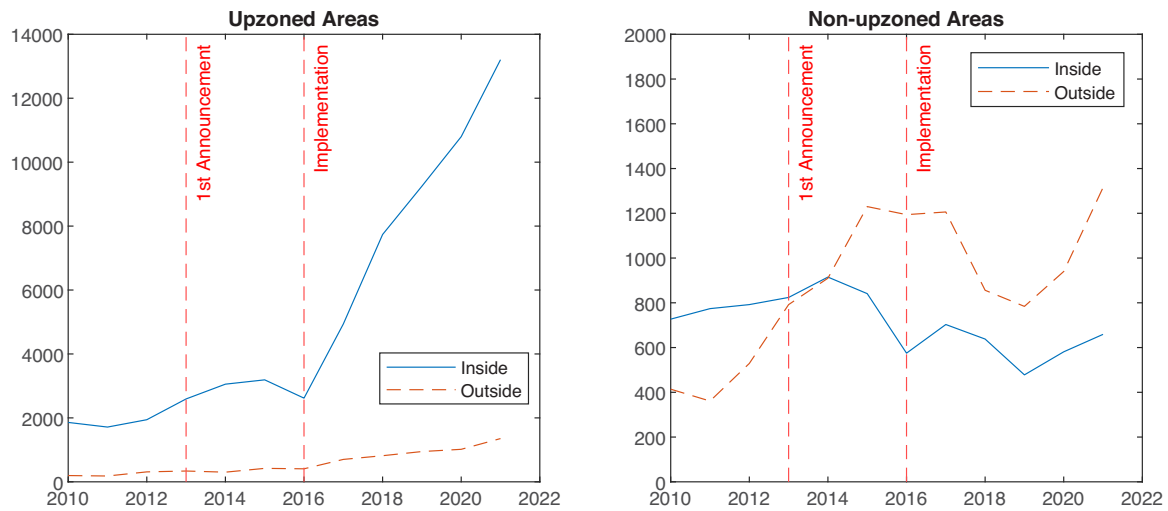


Fig. 4. Dwelling permits inside and outside the urban core, 2010–2021. Notes: Urban core depicted in Fig. 1.

difference in the level of the two series under the counterfactual (Kahn-Lang and Lang, 2020).

There is a notable decrease in the number of permits in non-upzoned areas after 2015. Permits follow a steady upward trend until 2015. Thereafter there is a discrete shift as permits trend downwards. This break in trend is statistically significant (refer to the Appendix for details). These outcomes are consistent with a negative spillover effect, as construction that would otherwise have occurred in non-upzoned areas may have been relocated to upzoned areas as a result of the policy. The outcome is mainly driven by a decline in detached dwelling permits, which lends further support to the negative spillover interpretation of the switch in trend in 2015.

Economic theories of urban development suggest that upzoning will encourage construction in desirable locations where zoning regulation was previously binding, such as areas close to job locations. The canonical Alonso-Muth-Mills (AMM) spatial equilibrium model of the monocentric city predicts that a relaxation of Land Use Regulations (LURs) will result in more housing close to the city to the center (Bertaud and Brueckner, 2005). To explore whether this is the case in Auckland, Fig. 4 divides the sample into urban core and non-core areas of Auckland (see Fig. 1 for the geographic delineation of the urban core and non-core areas). Consistent with this prediction, most of the increase in permits in upzoned areas is occurring in the urban core.

Figure 5 breaks down the upzoned areas into constituent Terrace Housing and Apartments (THA), Mixed Housing Urban, (MHU) and Mixed Housing Suburban (MHS) zones. Despite having more restrictive constraints than MHU and THA, it is unsurprising that MHS accounts for most of the increase in permits because it covers the largest geographic area.

4. Empirical model and results

Let $y_{i,j,t}$ denote the number of permits in zone j in area $i = 1, \dots, n$ in period $t = -T, \dots, 0, \dots, \bar{T}$, where T denotes the number of time series observations prior to the treatment, and \bar{T} denotes the number of time series observations post-treatment. The treatment occurs in period $t = 0$. We use $j = 0$ to indicate the control group (i.e, permits in non-upzoned areas) and $j = 1$ to signify the treatment group (permits in upzoned areas). The causal impact of upzoning is then estimated using a multi-period difference-in-differences (DID) specification of the form

$$y_{i,j,t} = \alpha_{i,j} + \sum_{s=-T, s \neq 0}^{\bar{T}} \phi_s \mathbf{1}_{s=t} + \sum_{s=-T, s \neq 0}^{\bar{T}} \beta_s \mathbf{1}_{s=t, j=1} + \varepsilon_{i,j,t} \quad (1)$$

where $\alpha_{i,j}$ are suburb-zone fixed effects, ϕ_s are period fixed effects, and $\mathbf{1}_{s=t}$ are indicators for each time period except the treatment period,

$t = 0$. $\mathbf{1}_{s=t, j=1}$ are indicators for each time period (except $t = 0$) interacted with a treatment indicator. Thus $\{\beta_s\}_{s=1}^{\bar{T}}$ represent the treatment effects over time from upzoning. The empirical estimates of these parameters capture the increase in permits in treatment areas relative to control areas in each period after upzoning is implemented. Following convention, estimates of $\{\beta_s\}_{s=-T}^{-1}$ will be used to assess the extent to which the parallel trends assumption holds prior to treatment. The period fixed effects ϕ_s measure changes in permits in the control group relative to the implementation period $t = 0$. They also capture common variation in permits across different zones and suburbs that is due to macroeconomic or city-wide shocks and policy changes.

We use “Statistical Areas” (SAs), which are similar to census tracts in the US, as the geographic unit of analysis indexed by i . SAs have a target population of between 2000 and 4000 people in cities (such as Auckland) and were delineated to reflect communities that interact socially and economically. There are 479 SAs in our sample.¹²

One of the strengths of DID is that spatial and time series variation in Land Use Regulations (LURs) can be used to control for a wide set of time invariant confounding factors. Although studies frequently employ spatial discontinuity designs to control for these confounders by assuming they are equally salient on either side of the boundary (Turner et al., 2017; Anagol et al., 2021), these approaches necessarily constrain the geographic area of analysis to areas spanning zone boundaries, making the empirical design ill-suited to aggregating treatment effects without imposing strong assumptions on the saliency of the policy intervention in areas distant to the boundary.

Permits are modeled in levels. As discussed above, levels seem more appropriate given the observed pre-treatment trends in upzoned and non-upzoned areas, which differ over time by a near constant amount until the Auckland Unitary Plan (AUP) is implemented. An additional benefit of modeling outcomes in levels is that it allows us to define counterfactual sets in terms of model parameters. By definition, spillovers

¹² SAs were introduced in 2018, as the previous classification system had not been revised since 1992. The previous statistical geographies no longer reflect current land use and population patterns. The revision was also implemented in order to align the geographic unit standards with international best practice. Population data from the previous census (conducted in 2013) and associated projections were used in the design of the 2018 boundaries. For additional details, see <https://www.stats.govt.nz/assets/Uploads/Retirement-of-archive-website-project-files/Methods/Statistical-standard-for-geographic-areas-2018/statistical-standard-for-geographic-areas-2018.pdf> [Accessed 1 March 2023]

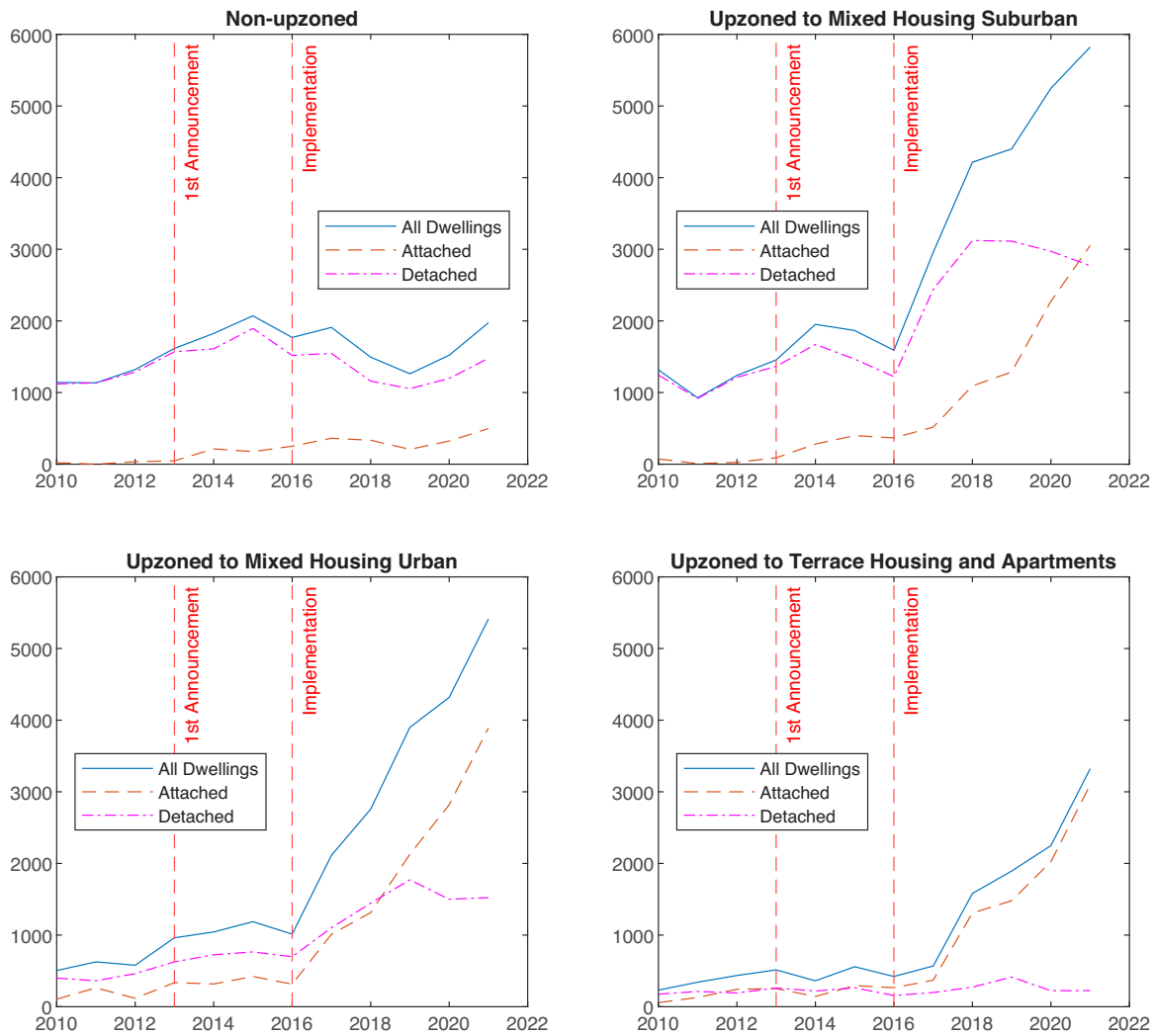


Fig. 5. Dwelling permits by residential zone, 2010–2021. Notes: Dwelling permits issued in non-upzoned and upzoned residential areas.

are measured in levels. For example, consider a spillover that generates $\epsilon \in \mathbb{R}^+$ fewer permits in non-upzoned areas – and ϵ more permits in upzoned areas – in the first treatment period. The corrected treatment effect would be $\hat{\beta}_1 - 2\epsilon$ and the corrected period fixed effect would be $\hat{\phi}_1 + \epsilon$. This direct mapping is lost or transformed if permits are instead modeled in logs or another non-linear transformation.

4.1. Selection of the treatment date

We use 2015 as the treatment date.¹³ There are two main reasons for this selection.

First, policy interventions can begin to manifest prior to the policy change if agents are notified of the change in advance. This is possible in the case of the AUP because the first version of the plan, with clear notification of the intent for dwelling intensification, was released in 2013, more than three years before the final version became operational (see Section 2 above). Negative spillovers can manifest as a decrease in permits in non-upzoned areas prior to 2016 if developers delay and shift planned construction to upzoned areas. The observed change in trend in non-upzoned areas depicted in Fig. 3 is consistent with a negative spillover: Permits trend upwards until reaching a peak in 2015, a year before policy is implemented in late 2016, before trending downwards to a nadir in 2019.

¹³ We use other dates in our set of robustness checks.

Second, setting the treatment date to coincide with the pre-intervention peak in non-upzoned permits yields more conservative estimates of treatment effects when accounting for negative spillovers via set identification. This is because the extrapolated linear trend that provides the basis for the set of counterfactual outcomes is steeper (see Section 5 below), resulting in a larger modeled spillover than that obtained under an alternative treatment date.

4.2. Results

Figure 6 shows OLS estimates of the coefficients alongside 95% confidence intervals (standard errors are clustered by SA). Recall that we set the treatment to occur in 2015. The top panel of the Figure displays results for all dwellings, while the middle and bottom panels display results for detached and attached dwellings, respectively.

Evidently there is no apparent trend in the estimated coefficients prior to treatment in any of the three samples, so the parallel trends assumption appears to hold. Further buttressing the DID framework, we find no evidence of anticipation in land prices or sales volumes prior to policy announcement, nor selection into treatment based on specifications that admit neighborhood-specific trends (see the robustness checks provided in Section 5.5.1). We proceed under the assumption that there is no confounding variable generating a difference in trends between treatment and control areas prior to policy implementation.

The response to the zoning change is immediate: The estimated treatment effects increase every year after implementation. By 2021, five

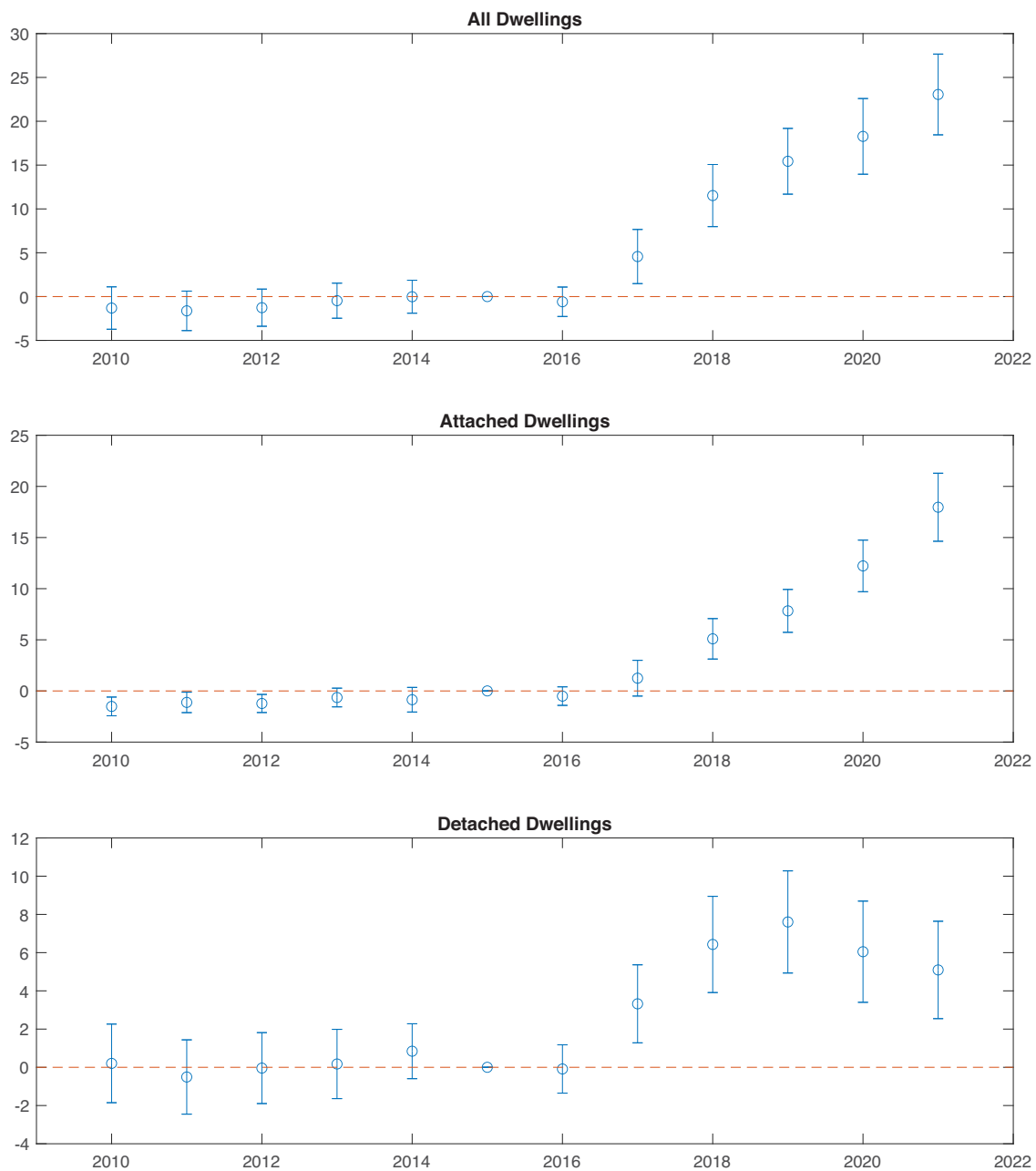


Fig. 6. Estimated treatment effects, 2010–2021. Notes: Estimated treatment effects (circles) and 95% confidence intervals (error bars). Treatment date is 2015. Outcome is permits per statistical area.

years after the policy was introduced, some 23.06 additional permits are issued, on average, in upzoned areas compared to non-upzoned areas in each of the 479 SAs. This would correspond to 11,044 permits across the city. Cumulating the corresponding figures for 2016 through 2021 yields 34,614 additional permits in upzoned areas compared to non-upzoned areas. Treatment effects for attached dwellings exceed detached from 2019 onwards. By 2021, the estimated treatment effect for attached is 17.96 permits, while for detached it is 5.09.

4.3. Spillover effects

It is possible that upzoning reallocated construction to upzoned areas that would have otherwise occurred in non-upzoned areas. This negative spillover effect would lead to an overstatement of the treatment effect,

since some of the permits in upzoned areas would have been issued in non-upzoned areas under the counterfactual of no policy change.

The time series plots of dwelling permits in Fig. 3 appear to be consistent with a negative spillover effect. There is a mild upward trend in permits built in non-upzoned areas until 2015, one year prior to policy implementation. Thereafter, there is a mild decrease until 2019. Meanwhile, permits in upzoned areas trend upwards from 2016 onwards. This pattern is consistent with a negative spillover effect that shifted construction from non-upzoned to upzoned areas as a result of the policy. The initial upward trend in the control group, and subsequent downturn from 2015 onwards, is most evident in the detached dwellings sample, which is also consistent with a negative spillover.

These patterns are also evident in the estimated period fixed effects (i.e., $\hat{\phi}_s$), which are depicted in Fig. 7. The coefficients measure changes in permits in the control group relative to the treatment period (2015).

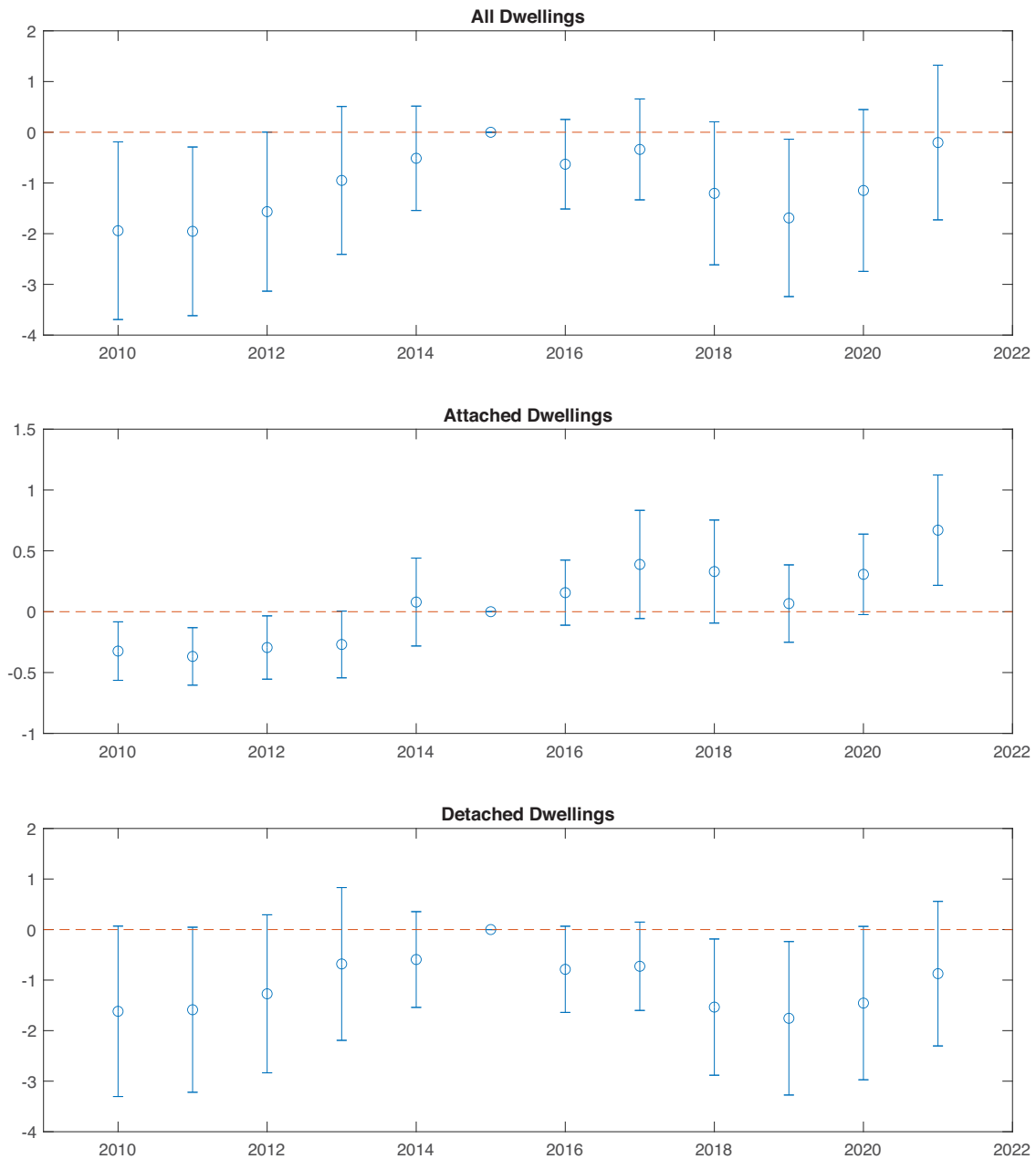


Fig. 7. Estimated period fixed effects, 2010–2021. Notes: Estimated period fixed effects (circles) and 95% confidence intervals (error bars). Outcome is permits per statistical area.

In the all dwellings sample, the coefficients trend upwards until 2015. Thereafter, they trend down until 2019.

To examine the potential mechanisms generating spillover effects, we specify a regression function that explains variation in the period fixed effects (PFEs) depicted in Fig. 7. To do so, we use a set of location-specific explanatory variables contained in the vector X_i , and fit the following regression to the data

$$y_{i,j,t} = \alpha_{i,j} + \sum_{s=-T,s \neq 0}^{\bar{T}} \phi_s \mathbf{1}_{s=t} + \sum_{s=-T,s \neq 0}^{\bar{T}} \mathbf{1}_{s=t} \gamma'_s X_i + \sum_{s=-T,s \neq 0}^{\bar{T}} \beta_s \mathbf{1}_{s=t,j=1} + \sum_{s=-T,s \neq 0}^{\bar{T}} \mathbf{1}_{s=t,j=1} \zeta'_s X_i + \varepsilon_{i,j,t},$$

where X_i is a vector of variables specific to Statistical Area (SA) i , comprising: (i) Manhattan distance to the CBD from the SA's centroid; (ii) Manhattan distance to the nearest highway onramp from

the centroid; (iii) undeveloped residential land area; and (iv) zoning concentration.

The sequence of parameter vectors $\{\gamma_s\}_{s=-T}^{\bar{T}}$ captures time series variation in control group permits that can be attributed to cross sectional variation in X_i . If a given variable contained in X_i accounts for the spillover effect, we anticipate the associated sequence of coefficients to mimic the pattern depicted in Fig. 7 – particularly the switch from an upward trend to a downward trend in 2015, which, as discussed above, is consistent with a negative spillover. (i) is included to examine whether this pattern is more prevalent in more distant areas, which would imply that spillovers manifest as a re-allocation of permits from distant exurbs to inner suburbs. (ii) is a conceptually similar variable, but uses highway network access points that facilitate access to all other regions of the city, not just the CBD, as the relevant measure of distance. (iii) is the amount of residential land in the SA that falls outside of the “ur-

ban extent” of Auckland (as defined at the time of the zoning reform), which is used as an approximation of developed land.¹⁴ It is included to examine whether the observed patterns in control group permits are more prevalent in locations with more undeveloped land, which would imply that spillovers manifest as a shift from greenfield housing development to redevelopment and infill housing. (iv) is the average minimum distance between upzoned and non-upzoned parcels within the SA,¹⁵ and it is included to examine whether there are local spillovers within the same neighborhood. The larger the measure, the greater the distance, on average, between upzoned and non-upzoned parcels, and thus the greater the concentration of zoning within the statistical area. If there are negative local spillover effects, whereby development is reallocated from non-upzoned to upzoned areas within suburbs, statistical areas with more concentrated zoning are more likely to experience a smaller (in magnitude) post-reform decrease in permits in control (non-upzoned) areas. If there are positive local spillover effects, statistical areas with more concentrated zoning are likely to experience a larger (in magnitude) post-reform decrease in permits in control areas.¹⁶

Figure 8 presents the point estimates of the PFEs ϕ_s and the elements of γ_s for each of the explanatory variables listed above. Each variable in X_i is standardized (but not demeaned), so the magnitude of each variable’s coefficient reflects its explanatory power. The y-axis span is uniform across all variables to facilitate comparability.

The initial upward and subsequent downward trend in the PFEs that is evident in Fig. 7 is gone, and the estimated coefficients are very close to zero, suggesting that our set of variables X_i explains much of the time series variation in permits in non-upzoned areas. Looking to the plots for the explanatory variables, the amount of undeveloped land area has the most explanatory power for the observed patterns in permits in non-upzoned areas over the time period. The pattern in the coefficients mimic those observed in the PFEs exhibited in Fig. 7, steadily increasing through to 2015, before falling to a nadir in 2019. The coefficients for the remaining variables are smaller in magnitude and statistically indistinguishable from zero in all periods.

The evidence is therefore consistent with the spillovers reallocating construction across different suburbs – specifically from relatively undeveloped suburbs to more developed suburbs – rather than within suburbs. In the following section, we present our method for accommodating spillovers that reallocate permits over large distances.

5. Set identification of treatment effects under spillovers

To account for spillover effects we adopt a confidence set identification approach by repurposing methods recently proposed in Rambachan and Roth (2023). First, we specify a set of plausible counterfactual scenarios based on observed pre-treatment trends in the control

group. We then test whether outcomes in the treatment group are significantly different from the set of counterfactuals.

Pre-treatment trends are frequently used to infer information about the counterfactual in DID frameworks. For example, RR (2023) propose extrapolating pre-treatment trends in estimated treatment effects to make inferences about counterfactual outcomes. Here, we propose extrapolating trends in control group outcomes to learn about the counterfactual.

Figure 9 superimposes a linear trend on the period fixed effects, which capture outcomes in the control group. The linear trend is only fitted to the pre-treatment sample (2010–2015), and passes through the coefficients in the first period and the treatment period (the latter is normalized to zero by convention). Extrapolating the linear trend into the treatment period yields 2.330 additional permits in the control group in the final period (since $\hat{\phi}_{-T} = -1.942$ and $2.330 = 1.942 \times \frac{6}{5}$). However, we actually observe 0.203 fewer permits in the control group in the final period (since $\hat{\phi}_T = -0.203$). Using the extrapolated trend as the counterfactual implies that the treatment effect in the final period is overstated by 2.533 ($= 2.330 + 0.203$) permits, or approximately 1213 permits across 479 statistical areas.

It is desirable to permit some margin for error when using pre-treatment trends to extrapolate a counterfactual scenario. This allows local deviations to potential nonlinearities in the trends. The approach suggested by RR (2023) is to adopt a set around the extrapolated trend. For example, Fig. 9 also includes a set of counterfactual scenarios in the control group such that there is a margin of error of ± 5 permits in the final period. This means that the control group counterfactual for 2021 lies anywhere between $-2.670 (= 2.330 - 5)$ and $7.330 (= 2.330 + 5)$ additional permits relative to the treatment period (2015). Note we specify the set lengths to grow linearly in $t \geq 1$, such that the sets are smaller for earlier periods.

Identification is then based on the difference between observed outcomes in treated areas and the set of counterfactual outcomes, which naturally gives rise to set (rather than point) identification of treatment effects. To formalize the set identification method, we partition $\beta = (\beta'_{pre}, \beta'_{post})'$, where $\beta_{pre} = (\beta_{-T}, \dots, \beta_{-1})'$ and $\beta_{post} = (\beta_1, \dots, \beta_T)'$. Following RR (2023), we decompose $\beta_{post} = \delta_{post} + \tau_{post}$, where τ_{post} is the true treatment effect, and δ_{post} is the difference between the treatment and control groups under the counterfactual scenario. The conventional assumptions required for DID analysis (including no spillovers) ensure that $\delta_{post} = 0$. The quantities δ_{post} and τ_{post} are unobserved and unidentified.

Example. Suppose that $\bar{T} = 1$ (one post-treatment period) and that upzoning only reallocated permits from control to treatment areas, so that no additional permits were generated. Then $\beta_1 = \delta_1$ and $\tau_1 = 0$.

RR (2023) use β_{pre} to generate a set of possible counterfactual outcomes when the parallel trends assumption ($\beta_{pre} = 0$) does not hold. For example, in the three period DID case ($\bar{T} = T = 1$), they discuss setting $\delta_1 = -\beta_{-1} \pm M$ for some $M \in \mathbb{R}^+$. The intuition is that observed pre-treatment trends in the treatment group relative to the control group are informative of post-treatment trends under the counterfactual. The case where $M = 0$ imposes a linear extrapolation, which is highly restrictive. Permitting general $M \in \mathbb{R}^+$ allows for nonlinear patterns within a set of counterfactual scenarios.

In our application we want to account for negative spillovers that cause the estimated treatment effects to overstate the true treatment effects. To do so, we need to place bounds on the size of the spillover. In this regard, pre-trends in the treatment effects β_{pre} are uninformative.¹⁷ Instead, we propose using pre-treatment trends in control group outcomes to bound counterfactual outcomes. We therefore partition $\phi =$

¹⁴ Urban extent is a geographical measure of Auckland’s developed urban area that excludes rural, peri-urban (i.e., semi-rural) and open space areas. It is based on cadastral land parcels. See Fredrickson (2014) for further description of the concept and classification methodology. Auckland Council has used development outside the urban extent to approximate the concept of greenfield development –see <https://www.aucklandcouncil.govt.nz/about-auckland-council/business-in-auckland/docsoccasionalpapers/the-brownfield-bounce-march-2018.pdf> [Accessed 1 March 2023]

¹⁵ For each upzoned residential parcel in the geographic unit, we calculate the Haversine distance to the nearest non-upzoned parcel; and for each non-upzoned parcel, we calculate the Haversine distance to the nearest upzoned parcel. Nearest parcels are not restricted to within the SA. We then average this minimum distance across all parcels within the SA. The measure builds on the empirical strategy used by Turner et al. (2017), who use distance to zoning boundaries to measure the external effects of zoning regulation.

¹⁶ Turner et al. (2017) present evidence consistent with positive spillover effects. They find that external effects of land use regulation on land values are negative but statistically insignificant in the US. A negative external effect indicates that residents prefer locations that allow higher density.

¹⁷ However, our approach to modeling counterfactual scenarios based on pre-treatment trends in the control group could easily be extended to incorporate trends in both the treatment and the control group. Under such a scenario, β_{pre} is informative and would be used to bound δ_{post} .

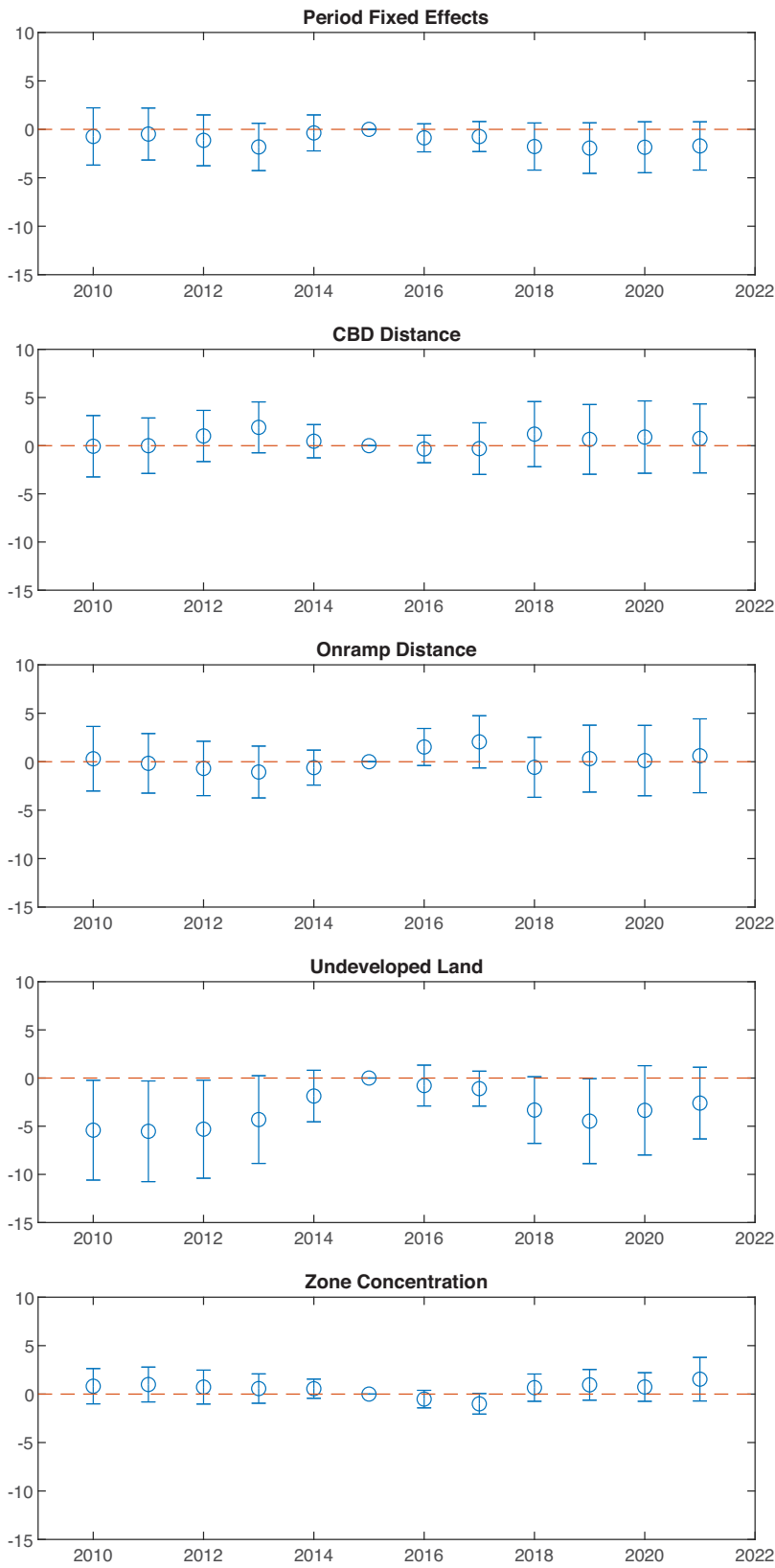


Fig. 8. Explaining variation in period fixed effects, 2010–2021. Notes: Point estimates (circles) and 95% confidence intervals (error bars).

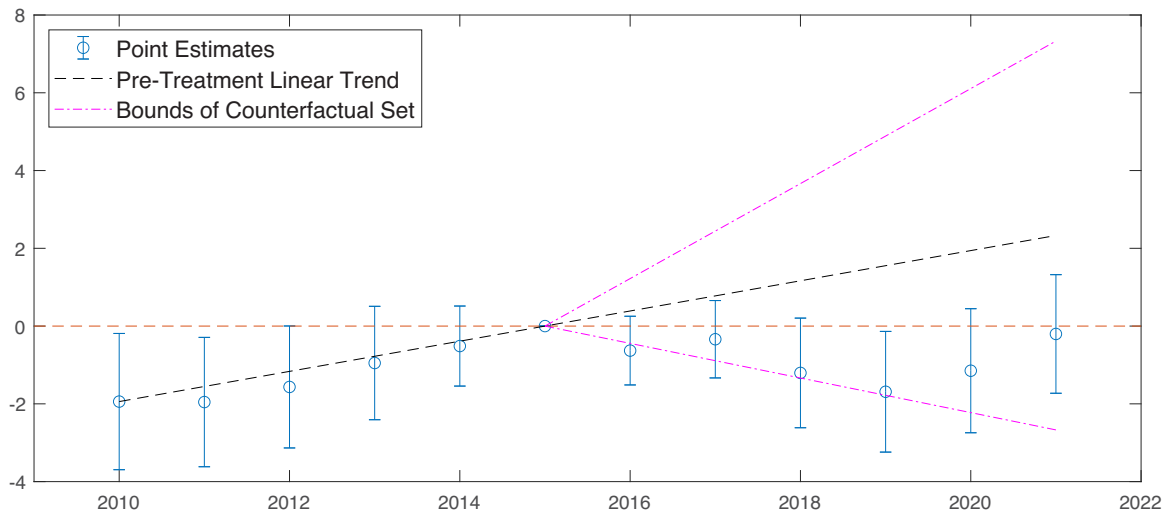


Fig. 9. Estimated period fixed effects and counterfactual sets, 2010–2021. *Notes:* The sequence of counterfactual sets allows a deviation of ± 5 permits (equivalent to ± 2395 permits over 479 statistical areas) from the linear trend in the final period (i.e. $M = 10$). The set of counterfactual outcomes in post-treatment periods is the space between the bounds.

$(\phi'_{pre}, \phi'_{post})'$, where $\phi_{pre} = (\phi_{-T}, \dots, \phi_{-1})'$ and $\phi_{post} = (\phi_1, \dots, \phi_T)'$, and use ϕ_{pre} to generate a set of counterfactuals.

Example. Suppose that $\bar{T} = \underline{T} = 1$ (so that we have three periods), $\phi_{-1} < 0$ (outcomes in the control group trend upwards prior to the treatment), and $\phi_1 < 0$ (outcomes in the control group trend downwards after the treatment). If control group outcomes remained on trend under the counterfactual, negative spillovers account for the observed downward deviation from trend after the policy is implemented. Then $\delta_1 = -2(\phi_{-1} + \phi_1)$ and $\tau_1 = \beta_1 + 2(\phi_{-1} + \phi_1)$.

In our application, we allow deviations from the linear trend, and we have several pre- and post-treatment observations. For each $t = 1, \dots, \bar{T}$ we bound the set of counterfactual outcomes by $-\hat{\phi}_{-T}t/\underline{T} \pm Mt/2\bar{T}$ for some $M \in \mathbb{R}^+$. Mt/\bar{T} therefore denotes the length of the counterfactual set for period t . The set for δ_t is then given by

$$\Delta_t = \left\{ \delta_t : \delta_t \in \left(-2\hat{\phi}_{-T}t/\underline{T} - Mt/\bar{T} - 2\hat{\phi}_t, -2\hat{\phi}_{-T}t/\underline{T} + Mt/\bar{T} - 2\hat{\phi}_t \right) \right\} \quad (2)$$

Having articulated the set of counterfactuals in (2), we can adopt the inferential architecture supplied by RR (2023). Because our counterfactual sets are convex and centrosymmetric, fixed length confidence intervals (FLCI) are consistent: For a given significance level $\alpha \in (0, 0.5]$, the coverage of FLCIs converge to $1 - \alpha$. Remaining technicalities of the method are provided in the Appendix.

Returning to our empirical application, Fig. 10 superimposes the FLCIs for $\alpha = 0.05$ and $M = 10$ on the conventional point estimates of the treatment effects depicted in Fig. 6. Notably, the confidence intervals sit above zero for 2018, 2019, 2020 and 2021, meaning we can reject the null of no treatment effect at the 5% significance level when allowing for the counterfactual sets depicted in Fig. 9. Note that each set is centered below the corresponding point estimate of the treatment effect, which is consistent with a negative spillover.

5.1. Set-identified treatments effects

We now consider set identification under various counterfactual set lengths, M . Figure 11 exhibits the set-identified treatment effects for $M = 4, 9$ and 14. In all cases, the identified set lies above zero for 2021. The largest counterfactual set, $M = 14$, spans $-2237 (= 479 \times (2.330 - 7))$ to 4469 ($= 479 \times (2.330 + 7)$) permits (recall that the linear trend is 2.330 in 2021). The upper bound of this set is approximately four times

the extrapolated linear trend (since $(7 + 2.330)/2.330 = 4.004 \approx 4$). This serves to pin down the magnitude of the increase in permits that would be necessary under a counterfactual set to render the treatment effects statistically insignificant. Specifically, we must allow for counterfactual scenarios that allow at least a four-fold increase in permits over the pre-treatment trend in order for the estimated treatment effects to become statistically insignificant. Such a scenario appears highly improbable. There is no concurrent policy change in the narrative record that could plausibly generate such a substantive increase in construction.

To help illustrate the substantive growth in permits that would be required under the counterfactual to render all of the treatment effects insignificant, Figure 15 in the Appendix presents the counterfactual sets when $M = 14$. The counterfactual set can even accommodate limited forms of exponential growth in permits over the six year post-treatment period, including a year-on-year growth rate of 13.68%.¹⁸

5.2. Set-identified treatments effects for attached and detached housing

This section applies the set identification approach to the subsamples of attached and detached housing.

5.2.1. Attached housing

First we fit the model to the subsample of attached dwelling permits. Figure 15 in the Appendix exhibits the estimated period fixed effects and the extrapolated linear trend fitted to the first period and the treatment period. Evidently, there is only a slight upward trend in attached dwelling permits prior to treatment, with the extrapolated trend yielding an additional 0.388 permits in 2021 compared to 2015. This implies an additional 186 ($= 0.388 \times 479$) permits in total across the 479 statistical areas under the extrapolated trend.

Figure 12 exhibits set-identified confidence intervals under various counterfactual sets ($M = 5, 10, 15$). The confidence interval for the largest $M = 15$ set sits above zero for 2021. Thus, treatment effects are statistically significant at the 5% level even when the counterfactual set spans an additional 3778 ($= (7.5 + 0.388) \times 479$) permits under the counterfactual scenario. This is about twenty times the 186 additional permits in the control area implied by the extrapolated linear trend.

¹⁸ In 2015 there were 2071 permits in non-upzoned areas. The upper bound allows up to 4469 permits in 2021, corresponding to an annual growth rate of 13.68%.

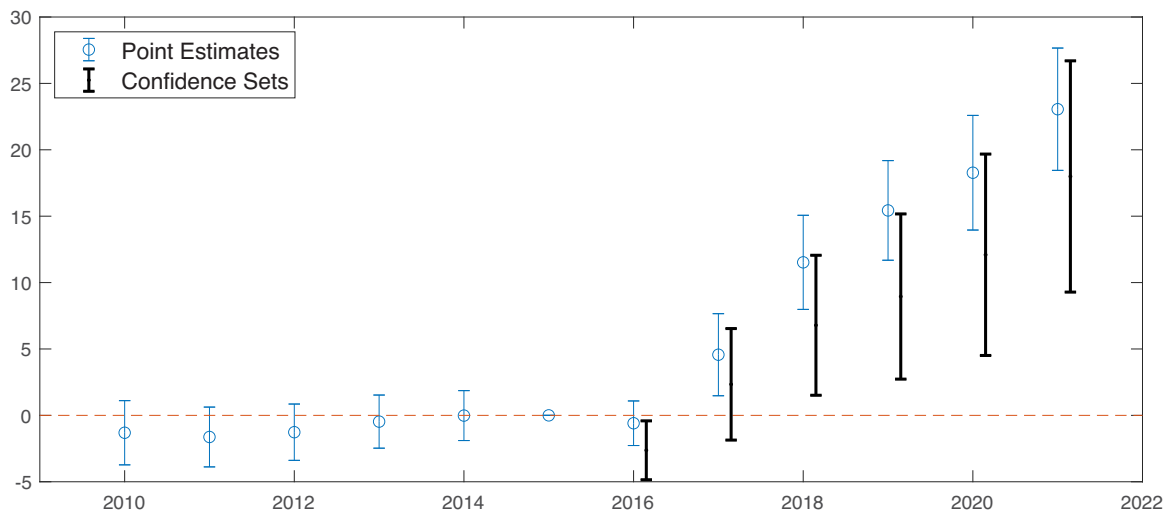


Fig. 10. Set-identified treatment effects, 2010–2021. Notes: Confidence sets obtained under the sequence of counterfactual sets depicted in Fig. 9. Treatment date is 2015. Outcome is permits per statistical area. Error bars denote 95% confidence intervals.

The conclusion seems unequivocal that upzoning has had a substantive impact on the construction of attached dwellings.

5.2.2. Detached housing

Next we fit the model to the subsample of detached dwelling permits. Figure 15 in the Appendix exhibits the estimated period fixed effects and the extrapolated linear trend fitted to the first period and the treatment period. Evidently, there is a marked increase in detached dwelling permits prior to treatment. The extrapolated trend then yields 1.942 additional permits in non-upzoned areas in 2021 compared to 2015. This would entail an additional 930 (= 1.942 × 479) permits in total.

Figure 13 exhibits the set-identified confidence intervals under various counterfactual sets ($M = 1, 2, 3$). In all cases, the confidence sets do not sit above zero, indicating acceptance of the null hypothesis of no positive treatment effect. The null is accepted even when M is set to the smallest possible value, zero (not pictured). We cannot conclude that upzoning had a substantive impact on the construction of detached dwellings.

5.3. Set-identified treatment effects by zone

We now analyze the impact of upzoning in each of the three constituent treatment zones separately. Figure 16 in the Appendix exhibits point estimated treatment effects for areas upzoned to Terrace Housing and Apartments (THA), Mixed Housing Urban, (MHU) and Mixed Housing Suburban (MHS). Treatment effects in all three areas are generally positive and statistically significant in the post-treatment period. There is some evidence of a downward pre-treatment trend for THA. The treatment effects in this area may therefore be under-estimated.

We also consider set identification of confidence intervals. Under the set identification approach there is no obvious method to allocate spillovers from the control group to the treatment groups. For example, all of the spillover could be allocated to a single area, such as upzoned to MHS. This would be consistent with all of the construction in the control zone spilling over into areas upzoned to MHS, and none into the MHU and THA areas.

In the absence of an obvious alternative, we elect to allocate the spillover to each of the three zones according to baseline levels of construction in each treatment area prior to upzoning. Between 2010 and 2014, 15.2% of permits were in areas upzoned to THA, 30.4% were in upzoned to MHU, and 54.4% were in upzoned to MHS. Let w_j denote the weights in zone j . For each $t = 1, \dots, \bar{T}$, we have

$$\Delta_t = \left\{ \delta_t : \delta_t \in w_j \left(-2\hat{\phi}_{-T}t/T - Mt/\bar{T} - 2\hat{\phi}_t, -2\hat{\phi}_{-T}t/T + Mt/\bar{T} - 2\hat{\phi}_t \right) \right\}.$$

Figure 16 exhibits the results with $M = 14$, which is the largest set considered earlier. For areas upzoned to THA and MHU, the treatment effects are statistically significant for 2019, 2020 and 2021. Meanwhile, the estimated effects for areas upzoned to MHS are statistically insignificant at the 5% level (two-sided). This is due to the fact that the treatment effects are small relative to the proportion of permits issued in this area prior to the policy. For example, the treatment effect for upzoned to MHU was 9.03 in 2021, which is slightly more than the 8.468 treatment effect in MHS. However, areas upzoned to MHS is allocated 54% of the spillover, whereas MHU receives only 30%.

5.4. How many additional permits did upzoning enable?

To obtain point estimates of the impact of upzoning, confidence sets were constructed with the counterfactual sets restricted to the counterfactual trend, i.e. $M = 0$. This implies that the treatment effect is estimated by subtracting $-2\hat{\phi}_{-T}t/T - 2\hat{\phi}_t$ from $\hat{\beta}_t$ for each $t = 1, \dots, \bar{T}$. In other words, two-times the difference between the extrapolated linear trend and observed permits in the control group is subtracted from the estimated treatment effect. This difference represents the additional permits that would have occurred in the control group had the policy not been implemented. Figure 14 depicts these “spillover-adjusted” point estimates of the treatment effects.

The spillover-adjusted treatment effects for each year between 2016 and 2021 are -2.63, 2.34, 6.79, 8.95, 12.09 and 18.00, respectively, yielding a cumulative total of 45.53. This implies 21,808 (= 45.53 × 479) additional permits as a result of the upzoning policy. To contextualize this figure, it corresponds to 4.11% of the city’s extant housing stock.¹⁹ Applying historic completion rates suggests that 21,808 permits would result in 20,718 (= 21,808 × 0.95) to 21,590 (= 21,808 × 0.99) completed dwellings.²⁰ It is important to note, however, that permits in upzoned areas are still trending upwards as of 2021, so the full impact of the policy will likely not be known for several more years.

A point of caution should be made in interpreting these findings. Mounting any counterfactual such as an extrapolated linear trend or any

¹⁹ Statistics New Zealand estimates that there were 530,300 dwellings in Auckland by the end of 2016. Source: <https://www.stats.govt.nz/experimental/experimental-dwelling-estimates>, Table 8 [Accessed 1 March 2023].

²⁰ Building completions typically range between 95% and 99% outside of recessionary periods. Source: <https://www.stats.govt.nz/experimental/experimental-dwelling-estimates>, Table 5 [Accessed 1 March 2023].

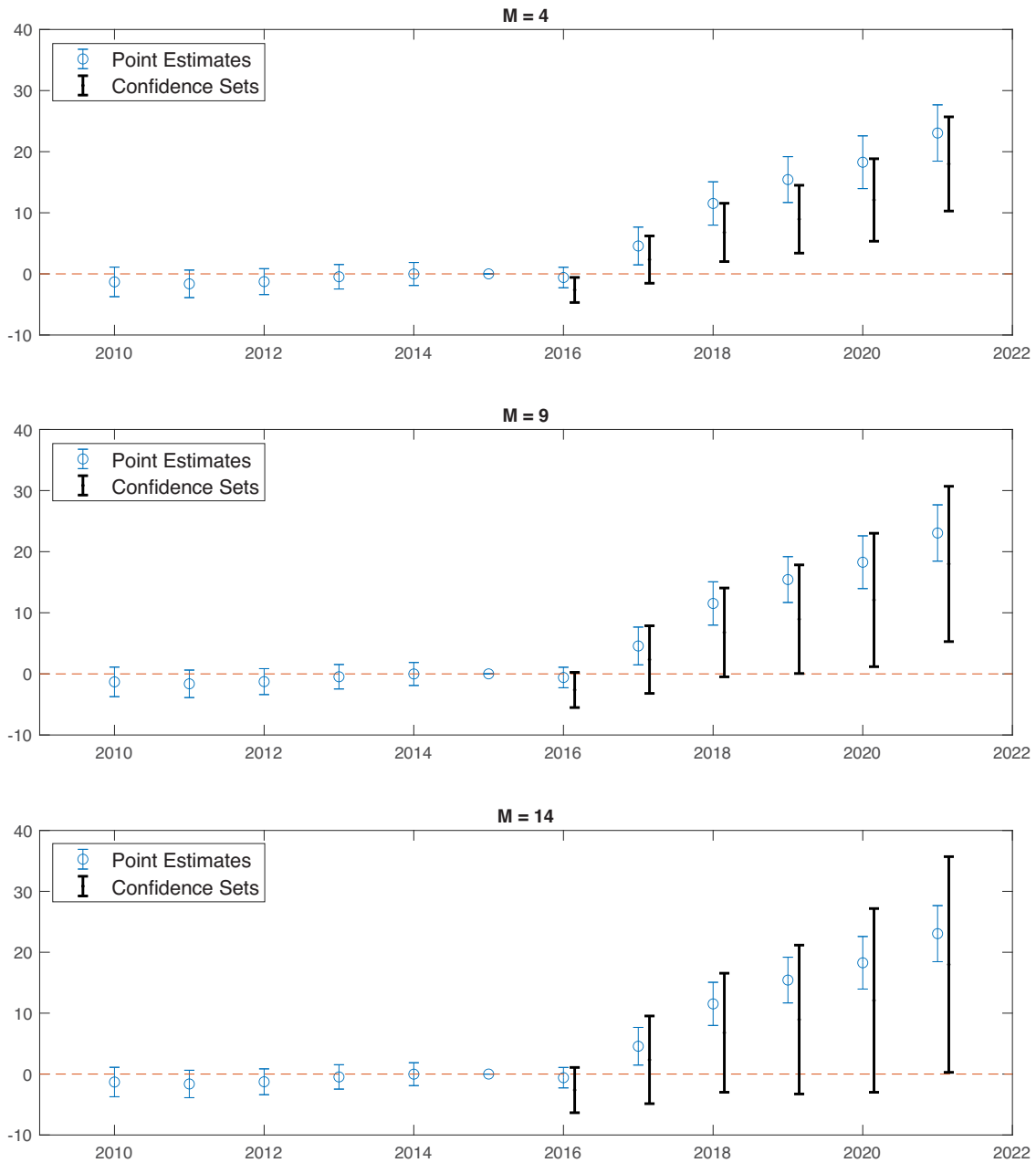


Fig. 11. Set-identified treatment effects, all dwellings, 2010–2021. Notes: M denotes the length of the set of counterfactual outcomes in the final period (2021). Treatment date is 2015. Outcome is permits per statistical area. Error bars denote 95% confidence intervals.

set of fixed points inevitably introduces potential misspecification due to the absence of an observable counterfactual scenario and the ambiguities in model selection. In this work a particular method for specifying a counterfactual has been used and point estimates will consequently be sensitive to changes in that specification. Importantly, set-identification mitigates such specification problems by constructing a set that covers a wide range of possible unobservable counterfactuals.

5.4.1. Residential zones

The analysis was repeated for the individual residential zones. We used the same weights as used in Section 5.3 above to allocate the spillovers to each of the three zones.

Figure 17 in the Appendix shows the results. For MHS, the spillover-adjusted treatment effects for each year have a cumulative total of 17.89 permits per statistical area, or 8570 additional permits across all statis-

tical areas (SAs). For MHU, the corresponding cumulative total is 22.95 permits per statistical area, or 10,991 across all SAs. Finally, for THA, the corresponding cumulative total is 15.12 permits per statistical area, or 7241 across all SAs. 27.0% of the overall increase in permits occurred in areas upzoned to THA, 41.0% in areas upzoned to MHU, and 32.0% in areas upzoned to MHS.²¹

²¹ Each area’s contribution to the overall increase in period s is calculated as $\hat{\beta}_s^j + (w_j + \frac{1}{3})\hat{\phi}_s$, where $\hat{\beta}_s^j$ denotes the treatment effect when $j \in \{THA, MHU, MHS\}$ is the treatment group. We use this attribution because $\sum_{j \in \{THA, MHU, MHS\}} (\hat{\beta}_s^j + \hat{\phi}_s) = \hat{\beta}_s^{ALL} + \hat{\phi}_s$ holds as an identity when the same control group is used for each area j (where $\hat{\beta}_s^{ALL}$ denotes the treatment effect for the aggregated areas).

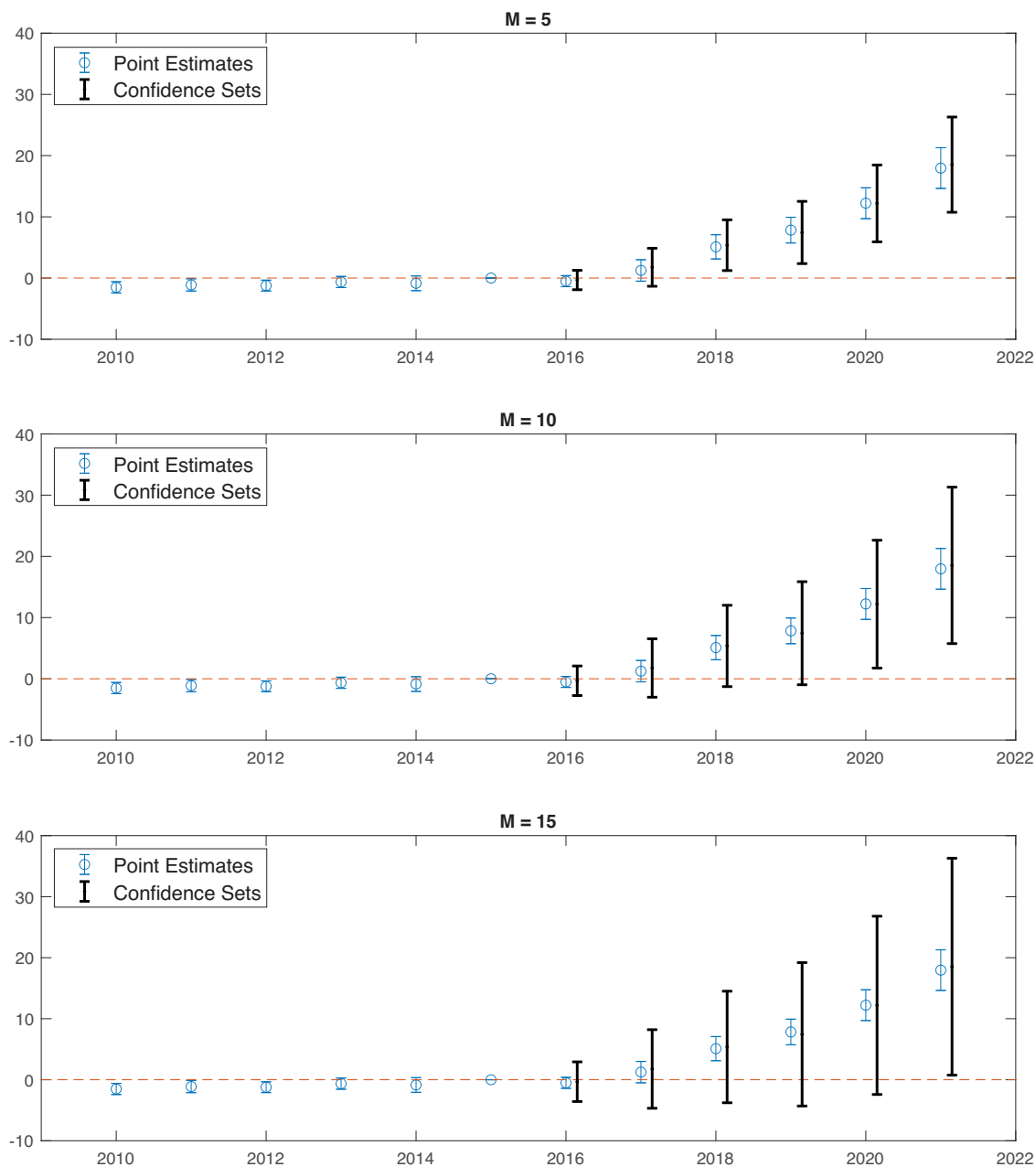


Fig. 12. Set-identified treatment effects, attached dwellings, 2010–2021. *Notes:* M denotes the length of the set of counterfactual outcomes in the final period (2021). Treatment date is 2015. Outcome is permits per statistical area. Error bars denote 95% confidence intervals.

5.5. Robustness checks

This section provides a brief description of results under various robustness checks.²² Results and additional details are provided in the Appendix.

5.5.1. Selection into treatment and policy anticipation

Although the parallel trend assumption appears to hold in our sample, we perform three robustness checks to examine whether upzoning was anticipated or whether there was selection into treatment.

The first two robustness checks are based on a geocoded dataset of residential housing sales. Because the policy was first announced in March 2013, we use 2012 as the treatment date, as the housing mar-

ket could respond to the policy prior to it being implemented. First, we fit the multi-period DID model to a spatial panel of (log) sales volumes. Interestingly, treatment effects become positive and statistically significant in 2017, suggesting sales activity increased in upzoned areas only after the policy was operationalized. Prior to 2017, coefficients are statistically indistinguishable from zero, indicating that there were no differential trends in sales volumes between upzoned or non-upzoned areas prior to policy implementation. Second, we fit the multi-period DID model to individual dwelling sales prices, wherein the site intensity ratio of the property is interacted with the treatment indicators in order to obtain treatment effects on land values, rather than the overall value of the property (Greenaway-McGrevy et al., 2021).²³ Abstracting from external effects of increased density or new construction, upzoned

²² We thank two anonymous referees for suggesting many of these robustness checks.

²³ The site intensity ratio is the value of improvements divided by the total value of the property, so that vacant land has an intensity ratio of zero. Valu-

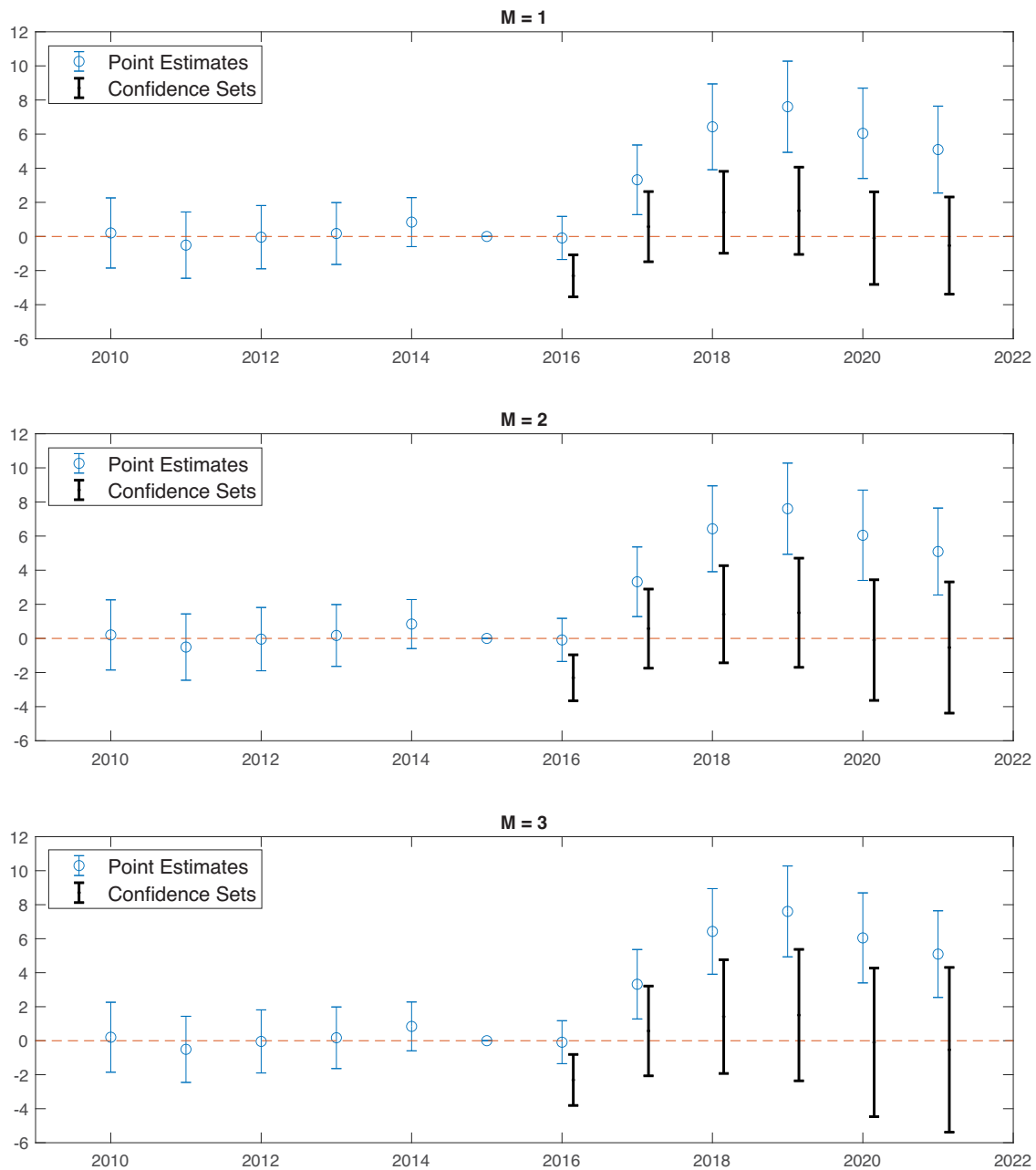


Fig. 13. Set-identified treatment effects, detached dwellings, 2010–2021. Notes: M denotes the length of the set of counterfactual outcomes in the final period (2021). Treatment date is 2015. Outcome is permits per statistical area. Error bars denote 95% confidence intervals.

land should be priced higher than non-upzoned land due to its greater potential floorspace capacity (Turner et al., 2017; Greenaway-McGrevy et al., 2021), all else equal. Relative to non-upzoned areas, land prices in upzoned areas increase from 2014 through to 2016, indicating that upzoning was capitalized into land prices soon after the policy was first announced in March 2013. Prior to 2013, coefficients are statistically indistinguishable from zero, indicating that there were no differential trends in land prices between upzoned or non-upzoned areas prior to policy announcement.

Another method to account for selection into treatment is to adopt an empirical model with period fixed effects that vary by geographic unit, which control for neighborhood-level demand trends. These trends

ations are local government estimates used for the purpose of levying property taxes. See Greenaway-McGrevy et al. (2021) for additional details.

may be a concern if policymakers direct development-intensive zones to neighborhoods that are experiencing upward or downward demand trends. The estimated treatment effects are robust to heterogeneous period fixed effects.

5.5.2. Alternative treatment dates and samples

We consider alternate specifications in which 2013, 2014 and 2016 are used as the date of treatment. Point estimates of treatment effects and spillover-adjusted treatment effects are reported in the Appendix. Estimates of the cumulative increase in the number of permits are slightly larger when alternative treatment dates are used, which is unsurprising given that permits in non-upzoned areas peaked in 2015.

We also consider empirical designs in which treatment and control areas are delineated differently. In the first, THA, MHU and MHS zones are the treatment group and SH areas are the control group. This ap-

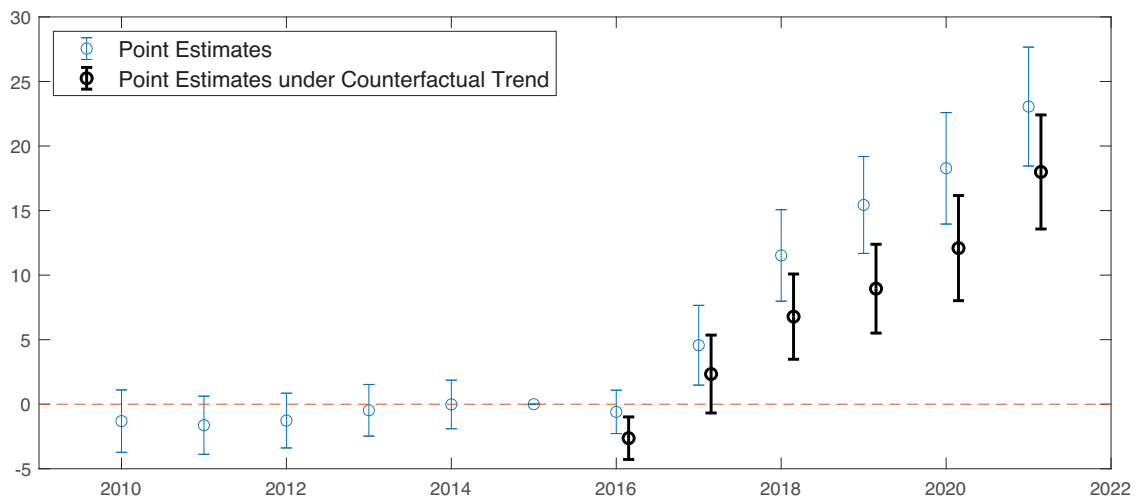


Fig. 14. Estimated treatment effects under counterfactual trend, all dwellings, 2010–2021. *Notes:* Estimates under the counterfactual trend are obtained by restricting the lengths of the counterfactual sets to zero, i.e. $M = 0$. Treatment date is 2015. Outcome is permits per statistical area. Error bars denote 95% confidence intervals.

proach ignores whether the floor area ratio (FAR) has changed. In the second, downzoned areas, where the FAR decreased, are excluded from the analysis. Our results and findings from these designs do not change substantively. Finally, we also consider a larger sample that includes permits issued under Special Housing Areas (see Section 2). Again, our findings and conclusions do not substantively change, although the point estimate of the net increase in permits under the linear trend counterfactual decreases to 17,957.

6. Discussion

The empirical findings show strong evidence to support the conclusion that upzoning raised dwelling construction in the city of Auckland. Set-identified treatment effects remain statistically significant even under counterfactual sets that include an implausibly large, four-fold increase in the trend rate of construction in control areas under the counterfactual of no-upzoning. The data also reveal that much of this increase is in the form of the more capital intensive, attached (or multifamily) structures in the inner suburbs of the city.

These findings are a positive sign for proponents of upzoning as a solution to unresponsive housing supply, particularly when compared to recent studies that show that zoning reforms have not had a substantial impact on housing construction (Freemark, 2019; Limb and Murray, 2022). Although the present paper does not explicitly identify factors that mediate the efficacy of zoning reforms in different contexts, comparisons to other reforms may shed light on the underlying mechanisms and thereby assist in directing future research. For example, the large-scale upzoning in Auckland may have fostered greater competition in land supply to developers, resulting in lower upzoned land prices than would have occurred if the policy had instead been restricted to specific neighborhoods or transit corridors. Alternatively, demand for housing in Auckland has significantly outstripped supply over the past few decades, resulting in housing that is amongst the most expensive in the world when measured against local incomes: Auckland may have significantly more latent demand for housing than other cities. We anticipate future research will address these questions to further guide policymakers in the design and implementation of zoning reforms.

We conclude by noting that the impact of upzoning on housing construction and housing markets will continue to be felt over coming years. Permits for attached dwellings are still trending upwards and permits for detached dwellings remain significantly above their pre-upzoning average. In future work, and as new data become available, the impact of the policy on the housing stock will be updated and new research will

seek to determine the particular characteristics of parcels that predict the uptake of redevelopment. Such findings should be useful in assisting the design and refinement of future upzoning policies.

CRediT authorship contribution statement

Ryan Greenaway-McGrevy: Conceptualization, Investigation, Methodology, Software, Writing – original draft. **Peter C.B. Phillips:** Methodology, Supervision, Writing – review & editing.

Appendix A.

The Appendix provides additional details concerning the Auckland Unitary Plan (AUP), the methods employed in our empirical work, with additional findings and robustness checks.

A1. Background and detailed timeline of the Auckland unitary plan

Prior to 2010, the greater Auckland metropolitan region comprised seven city and district councils: Auckland City Council, North Shore City Council, Waitākere City Council, Manukau City Council, Rodney District Council, Papakura District Council, and Franklin District Council. On 1 November 2010, Auckland Council (AC) was formed when the eight previous governing bodies in the region were amalgamated. Legislation was also passed by the central government requiring AC to develop a consistent set of planning rules for the whole region under the Local Government Act 2010. This set of planning rules is embodied in the Auckland Unitary Plan (AUP).

Key dates in the development and implementation of the AUP are as follows:

- 15 March 2013: AC releases the draft AUP. The next 11 weeks comprised a period of public consultation, during which AC held 249 public meetings and received 21,000 items of written feedback.
- 30 September 2013: AC released the Proposed AUP (PAUP) and notified the public that the PAUP was open for submissions. More than 13,000 submissions (from the public, government, and community groups) were made, with over 1.4 million separate points of submission.
- April 2014 to May 2016: an Independent Hearings Panel (IHP) was appointed by the central government, which subsequently held 249 days of hearings across 60 topics and received more than 10,000 items of evidence.

- 22 July 2016: the IHP set out recommended changes to the PAUP. One of the primary recommendations was the abolition of minimum lot sizes for existing parcels. The AC considered and voted on the IHP recommendations over the next 20 working days. On 27 July the public could access and view the IHP’s recommendations.
- 19 August 2016: AC released the “decisions version” of the AUP, including the new zoning maps. Several of the IHP’s recommendations were voted down, including a IHP recommendation to abolish minimum floor sizes on apartments. However, the abolition of minimum lot sizes for existing parcels was maintained. This was followed by a 20-day period for the public to lodge appeals on the decisions version in the Environment Court. Appeals to the High Court were only permitted if based on points of law.
- 8 November 2016: A public notice was placed in the media notifying that the AUP would become operational on 15 November 2016.
- 15 November 2016: AUP becomes operational. There were two elements of the AUP that were not fully operational at this time: (i) any parts that remain subject to the Environment Court and High Court under the Local Government Act 2010; and (ii) the regional coastal plan of the PAUP that required Minister of Conservation approval.

All versions of the AUP (“draft”, “proposed”, “decisions” and “final”) could be viewed online.

A2. Upzoning classification

For each geocoded permit in our sample, we identify the zone that previously applied to the permit’s location using GIS databases for each of the seven city and district plans. Then, for each of the approximately 115 residential zones (across the seven councils) that previously existed, we calculate the floor area ratio (FAR) based on the height limits and site coverage ratios in the district and city plans. Height limits are translated into storey limits based on building on with minimum 0.6 m ground clearance, 2.68 m per storey, and at least 2 m for a roof. The maximum FAR is then calculated as the product of the effective storey limit and the site coverage ratio. Permits that are located in areas that were previously not zoned as residential (business, rural, or open space) are classified as upzoned since they have been converted to enable residential housing.

We can apply the same algorithm to identify the zones of individual land parcels in order to quantify the amount of upzoned land. We obtain GIS information on land parcels for November 2016, when the AUP was operationalized. We then assign each parcel to a pre- and post-AUP zone using the polygon’s centroid, and apply the same algorithm described above to classify upzoned parcels. This process allows us to quantify the amount of upzoned land, as discussed in Section 2.

The parcel dataset is also used to calculate the amount of undeveloped residential land and the zoning concentration measure for each SA. These measures are used in the spillover analysis in Section 4.3. Parcels are also used to repair consents with geocoordinates that fall onto road frontages by finding the nearest parcel to the geocoordinate. This repair affects less than 2% of our sample.

A3. Structural break in trend in permits

For $j = 0$ (i.e., control group), we estimate

$$y_{i,j,t} = \alpha_{i,j} + \beta_j 1_{t \geq 1} + \delta_j t + \gamma_j 1_{t \geq 1} t + \varepsilon_{i,j,t}$$

where recall that $y_{i,j,t}$ is the number of permits in zone j in area i in year t , $1_{t \geq 1}$ denotes an indicator set to one for time periods after the treatment date (2015). OLS estimates are $\hat{\beta}_0 = 1.960$ (t -statistic = 1.383), $\hat{\delta}_0 = 0.418$ (t -stat = 3.0100) and $\hat{\gamma}_0 = -0.441$ (t -stat = -2.175), indicating a statistically significant reduction in the upwards linear trend after 2015. t -

statistics are based on panel data Newey-West standard errors with the temporal bandwidth set to two periods.

A4. Set identification

This section is based on Section 3.1 of RR (2023). Let $\theta = l' \tau_{post}$ be a linear combination of the treatment parameters of interest, where $l \in \mathbb{R}^T$. For example, if we are interested in the treatment effect in the final period, $l = (0, \dots, 0, 1)'$. Next, let $\hat{\lambda}_n$ be a relevant m -subvector of the estimate $\hat{\Lambda}_n = (\hat{\phi}'_n, \hat{\beta}'_n)'$, where $\hat{\lambda}_n \sim \mathcal{N}(\lambda, \Sigma_n)$. That is, there exists a full column rank $(\underline{T} + \bar{T}) \times m$ selection matrix \mathbf{J} such that $\hat{\lambda}_n = \mathbf{J}' \hat{\Lambda}_n$. The choice of $\hat{\lambda}_n$ depends on both the parameter of interest θ and the counterfactual set. A specific example is given below. We similarly define $\lambda = \mathbf{J}' \Lambda$, where $\Lambda = (\phi', \beta')'$ can be decomposed as follows

$$\Lambda = \begin{bmatrix} \phi_{pre} \\ \phi_{post} \\ \beta_{pre} \\ \beta_{post} \end{bmatrix} = \begin{bmatrix} \phi_{pre} \\ \phi_{post} \\ \beta_{pre} \\ \delta_{post} \end{bmatrix} + \begin{bmatrix} 0 \\ 0 \\ 0 \\ \tau_{post} \end{bmatrix} =: \delta + \tau.$$

The decomposition above accords with that given for β in (2) of RR (2023). We simply extend their framework to include ϕ in the parameter space of interest.

We consider FLCIs based on affine estimators of θ of the general form

$$C_{\alpha,n}(a, v, \chi) = (a + v' \hat{\lambda}_n) \pm \chi,$$

where α and χ are scalars, $v \in \mathbb{R}^m$, and $\alpha \in (0, 0.5]$ denotes a significance level. We choose a and v to minimize

$$\chi_n(a, v; \alpha) = \sigma_{v,n} \cdot cv_\alpha(\bar{b}(a, v) / \sigma_{v,n}),$$

where $\sigma_{v,n} = \sqrt{v' \Sigma_n v}$, and $cv_\alpha(\cdot)$ denotes the $1 - \alpha$ quantile of the folded normal distribution with unit variance, $|\mathcal{N}(\cdot, 1)|$. The quantity $\bar{b}(a, v)$ denotes the worst-case bias of the affine estimator for a given a and v , namely

$$\bar{b}(a, v) := \sup_{\delta \in \Delta, \tau_{post} \in \mathbb{R}^T} \left| a + v' (\mathbf{J}' \delta + \mathbf{J}' \tau) - l' \tau_{post} \right|, \quad (3)$$

where Δ denotes the set of permissible values of δ articulated under the counterfactual.

To demonstrate the method in more detail, we take $\theta = \tau_{\bar{T}}$ (so that interest is focused on the final treatment effect) and obtain the set of permissible values of $\delta_{\bar{T}}$ from (2):

$$\Delta_{\bar{T}} = \left\{ \delta_{\bar{T}} : \delta_{\bar{T}} \in \left(-2\hat{\phi}_{-\bar{T}} \bar{T} / \underline{T} - M - 2\hat{\phi}_{\bar{T}}, -2\hat{\phi}_{-\bar{T}} \bar{T} / \underline{T} + M - 2\hat{\phi}_{\bar{T}} \right) \right\}, \quad (4)$$

Thus $\lambda = (\phi_{-\bar{T}}, \phi_{\bar{T}}, \beta_{\bar{T}})$ is the subvector of parameters of interest. The affine estimator is then defined on $\hat{\lambda}_n = (\hat{\phi}_{-\bar{T}}, \hat{\phi}_{\bar{T}}, \hat{\beta}_{\bar{T}})$, and (3) can be more succinctly expressed as

$$\bar{b}(a, v) := \sup_{\delta_{\bar{T}} \in \Delta_{\bar{T}}, \tau_{\bar{T}} \in \mathbb{R}} \left| a + v' \left(\begin{bmatrix} \hat{\phi}_{-\bar{T}} \\ \hat{\phi}_{\bar{T}} \\ \delta_{\bar{T}} \end{bmatrix} + \begin{bmatrix} 0 \\ 0 \\ \tau_{\bar{T}} \end{bmatrix} \right) - \tau_{\bar{T}} \right|.$$

A5. Robustness checks

A5.1. Sales

We fit the multi-period DID model to the log of residential housing sales. We set 2013 as the treatment date, since the policy is first announced in March 2013. Figure 21 shows that there is no significant increase in sales prior to implementation in 2016.

A5.2. Land prices

Let $p_{i,t}$ denote the log sales price of house i in year t . Our empirical model is

$$p_{i,t} = \delta'_0 Z_i + \sum_{s=-T, s \neq 0}^{\bar{T}} \mathbf{1}_{s=t} \delta'_s Z_i + \mathbf{1}_{i \in u} \beta'_{0,j} Z_i + \sum_{s=-T, s \neq 0}^{\bar{T}} \mathbf{1}_{s=t} \mathbf{1}_{i \in u} \beta'_{s,j} Z_i + \varepsilon_{i,t}, \tag{5}$$

where $Z_i = (1, z_i, X'_i)$, and where z_i is site intensity, and X_i is a vector of standardized control variables, including floorspace, building age, number of garages, Manhattan distance to CBD, distance to nearest highway onramp, and distance to the nearest rapid transit station. The sample is a repeated sample of cross sections, since properties are not sold in every year of the sample period. $\mathbf{1}_{i \in u}$ is an indicator equal to one if house i is located in an upzoned area.

Site intensity is the ratio of improvements to total value of the property. Valuations are constructed by Auckland Council for levying property taxes. Vacant land, and properties with improvements valued at zero, have a site intensity of zero. Thus, the average change in the land price differential between upzoned and non-upzoned properties in period s is given by the first element of $\beta_{s,j}$, holding all else equal. We plot point estimates of these coefficients in Figure 22, both with and without control variables in the model specification. Land prices in upzoned areas increase relative to non-upzoned areas soon after announcement, but not before, indicating that the policy was not anticipated by the market. The model with controls indicates that land prices in upzoned areas increased by between 20 and 25% relative to non-upzoned areas, holding all else constant.

A5.3. Local period fixed effects

We estimate

$$y_{i,j,t} = \alpha_{i,j} + \sum_{s=-T, s \neq 0}^{\bar{T}} \phi_{i,s} \mathbf{1}_{s=t} + \sum_{s=-T, s \neq 0}^{\bar{T}} \beta_s \mathbf{1}_{s=t, j=1} + \varepsilon_{i,j,t} \tag{6}$$

where the period fixed effects $\phi_{i,s}$ are now indexed by the statistical area i . Figure 20 exhibits estimated treatment effects.

A5.4. Alternative treatment dates

Figure 18 exhibits estimated treatment effects when 2013, 2014 or 2016 is used as the treatment date instead of 2015. Constraining the counterfactual to the linear trend results in 26,800, 23,544 and 31,808 additional permits when 2013, 2014 and 2016 are used as the treatment date, respectively. The corresponding upzoned differentials (i.e., the cumulative difference between upzoned and non-upzoned areas) for these three treatment dates are 34,406, 34,670 and 36,318 permits, respectively.

A5.5. Alternative treatment and control areas

Figure 19 exhibits estimated treatment effects under different delineations of treatment and control areas. In the top panel, THA, MHU and MHS zones comprise the treatment areas, and SH is the control area. Constraining the counterfactual to the linear trend results in 20,195 additional permits. The upzoned differential is 35,052 permits. In the middle panel, downzoned areas are removed from the baseline sample, as defined in the main text. Constraining the counterfactual to the linear trend results in 20,307 additional permits. The upzoned differential is 34,897 permits. In the bottom panel, permits issued under Special Housing Areas are included in the sample. Constraining the counterfactual to the linear trend results in 17,957 additional permits, while the upzoned differential is 33,137 permits. Under set identification (not pictured), the confidence sets remain statistically significant even when allowing for a more than three-fold increase in the pre-treatment trend in non-upzoned areas.

A6. Additional figures

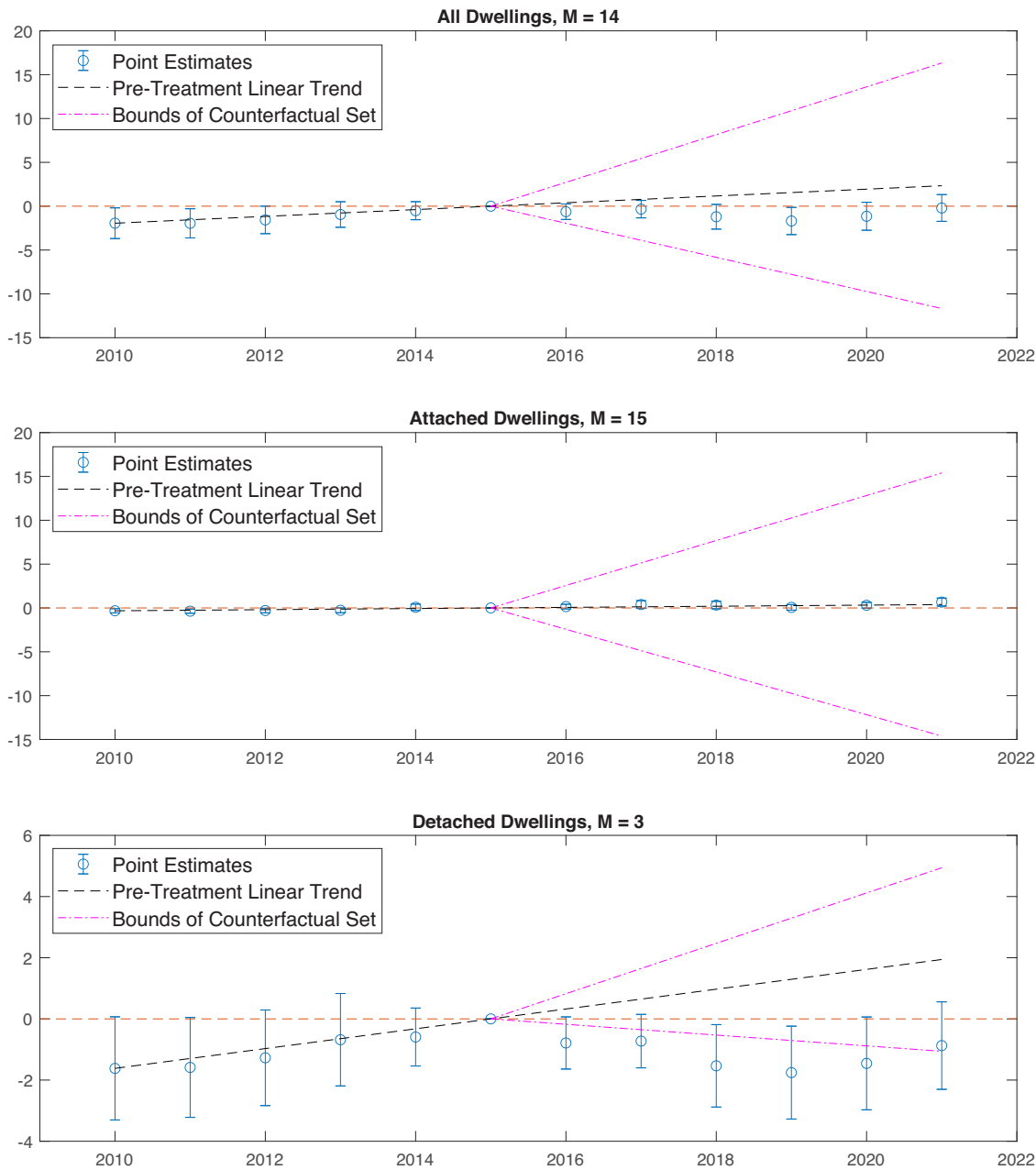


Fig. 15. Estimated period fixed effects and post-treatment counterfactual sets, 2010-2021. *Notes:* M denotes the length of the counterfactual set in the final period (2021). The set of counterfactual outcomes in post-treatment periods is the space between the bounds.

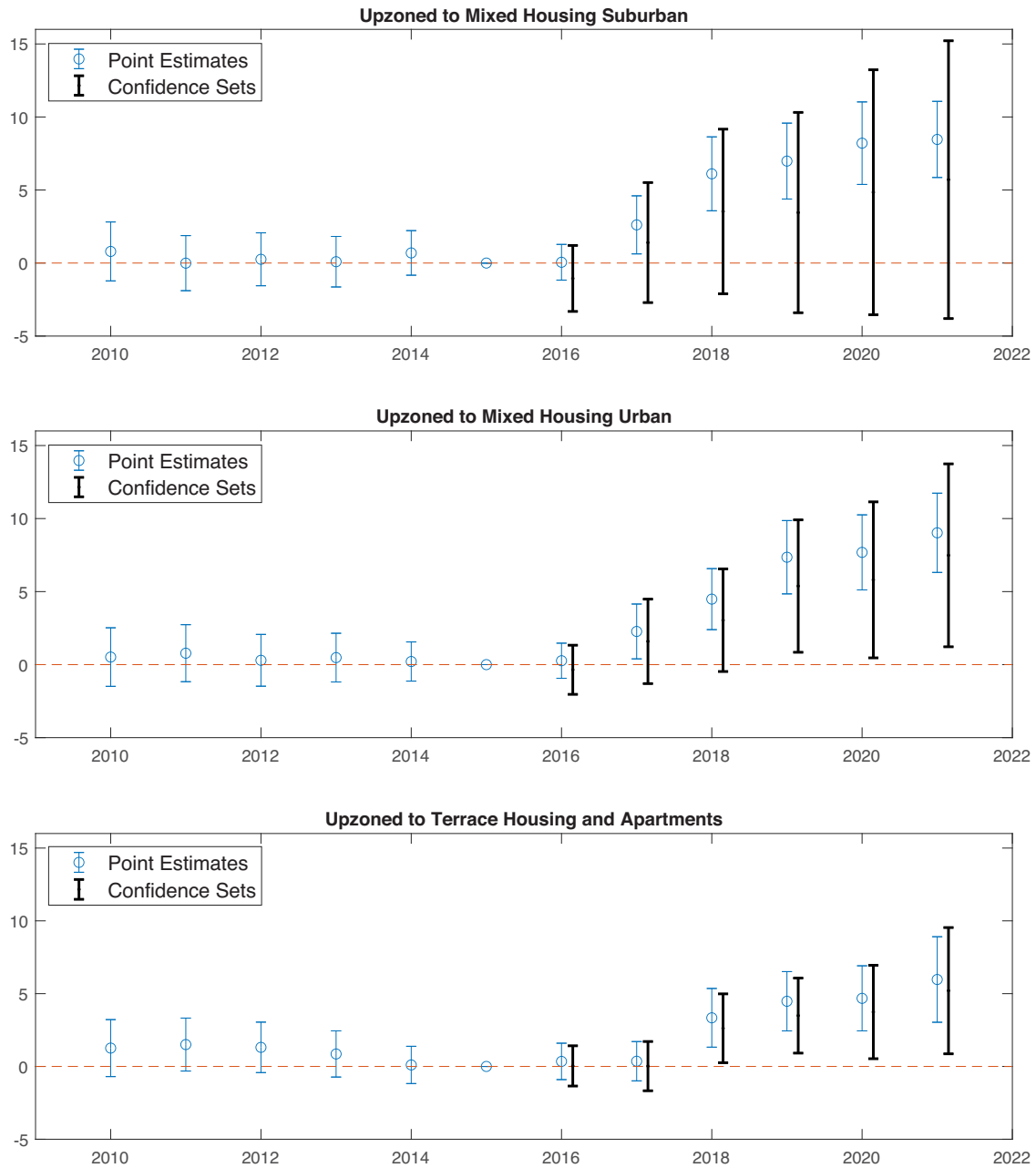


Fig. 16. Set-identified treatment effects by residential zone, 2010-2021. *Notes:* The length of the counterfactual set for the final period (2021) is set to 14, i.e. $M = 14$. Treatment date is 2015. Outcome is permits per statistical area. Error bars denote 95% confidence intervals.

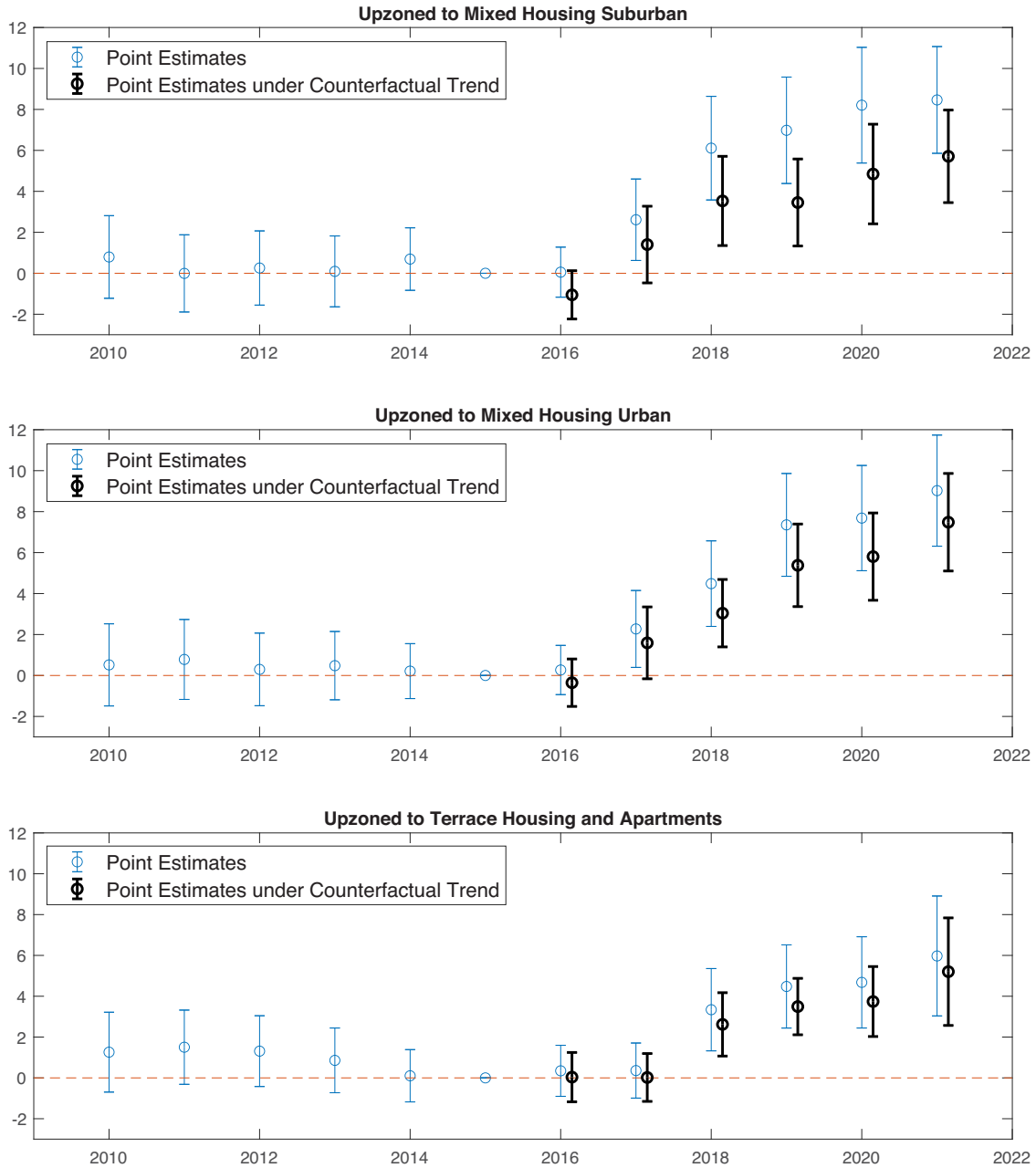


Fig. 17. Estimated treatment effects by residential zone under counterfactual trend, 2010-2021. Notes: Estimates under the counterfactual trend are obtained by restricting the lengths of the counterfactual sets to zero, i.e. $M = 0$. Outcome is permits per statistical area. Error bars denote 95% confidence intervals.

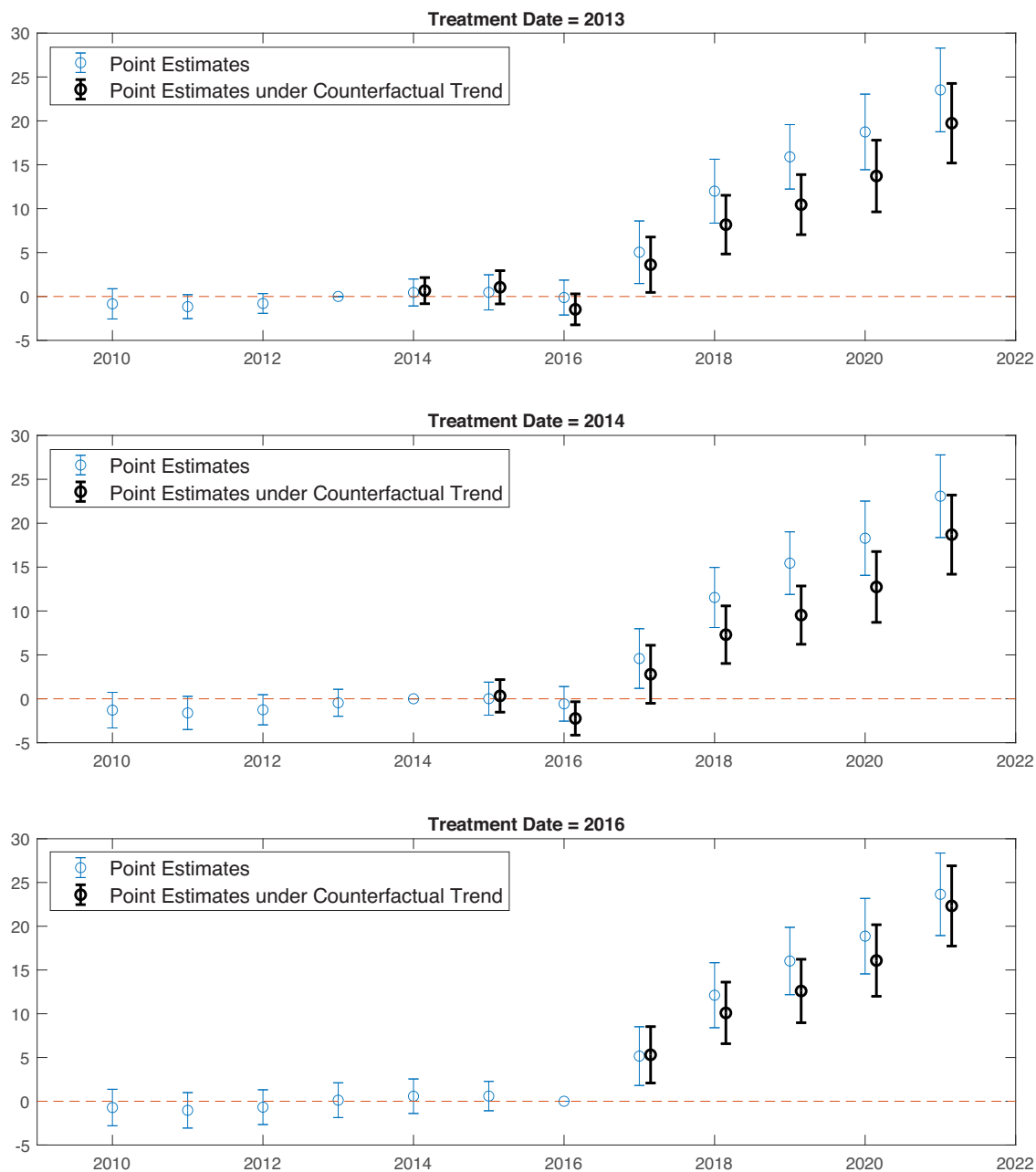


Fig. 18. Estimated treatment effects under alternative treatment dates. Notes: Estimates under the counterfactual trend are obtained by restricting the lengths of the counterfactual sets to zero, i.e. $M = 0$. Outcome is permits per statistical area. Error bars denote 95% confidence intervals.

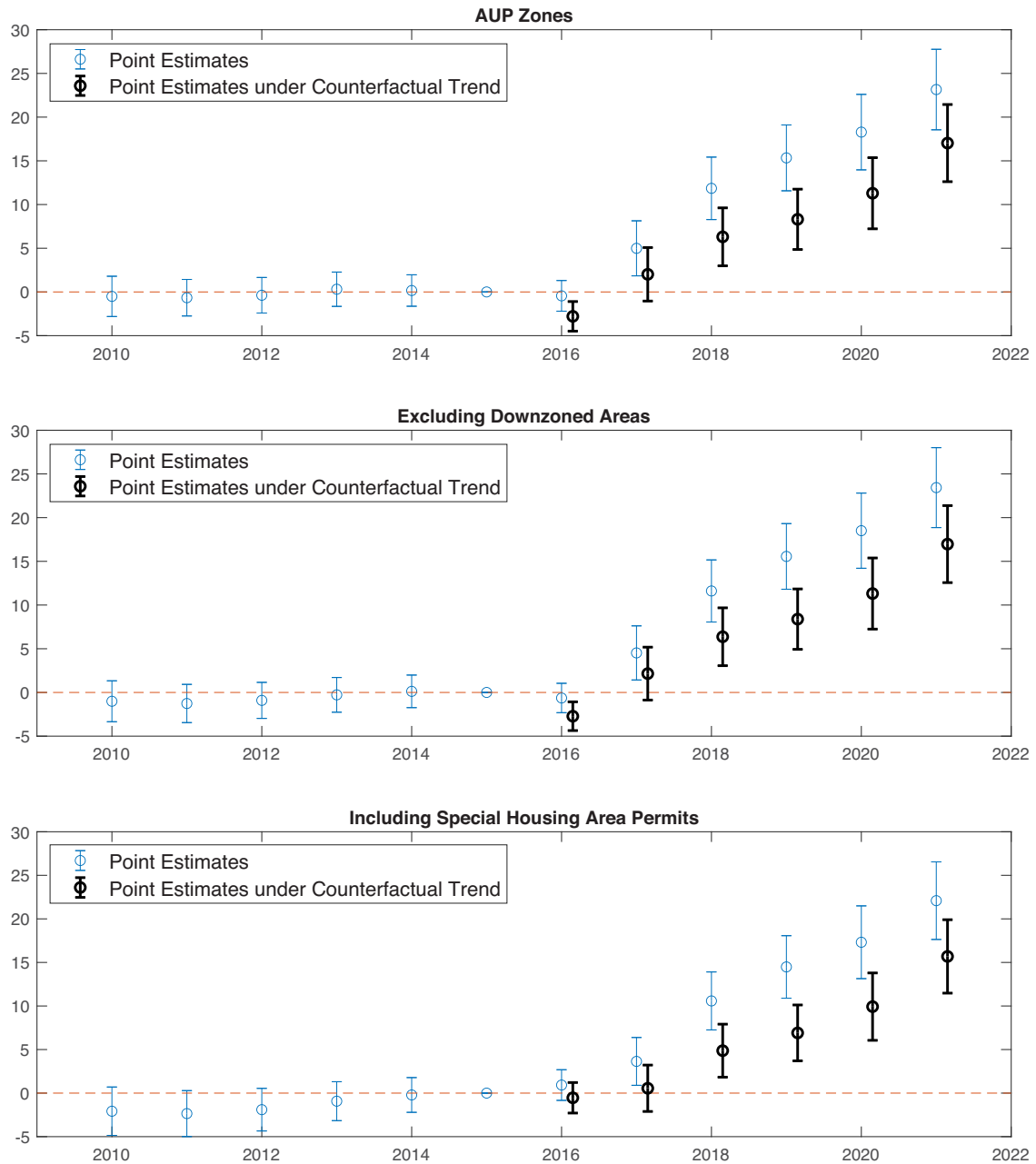


Fig. 19. Estimated treatment effects under alternative treatment and control areas. *Notes:* Estimates under the counterfactual trend are obtained by restricting the lengths of the counterfactual sets to zero, i.e. $M = 0$. Top: Terrace Housing and Apartments, Mixed Housing Urban and Mixed Housing Suburban zones comprise the treatment group, and the Single House zone is the control group. Middle: Downzoned areas removed from the sample. Bottom: Permits issued under special housing areas (see Section 2) are included in the sample. Outcome is permits per statistical area. Error bars denote 95% confidence intervals.

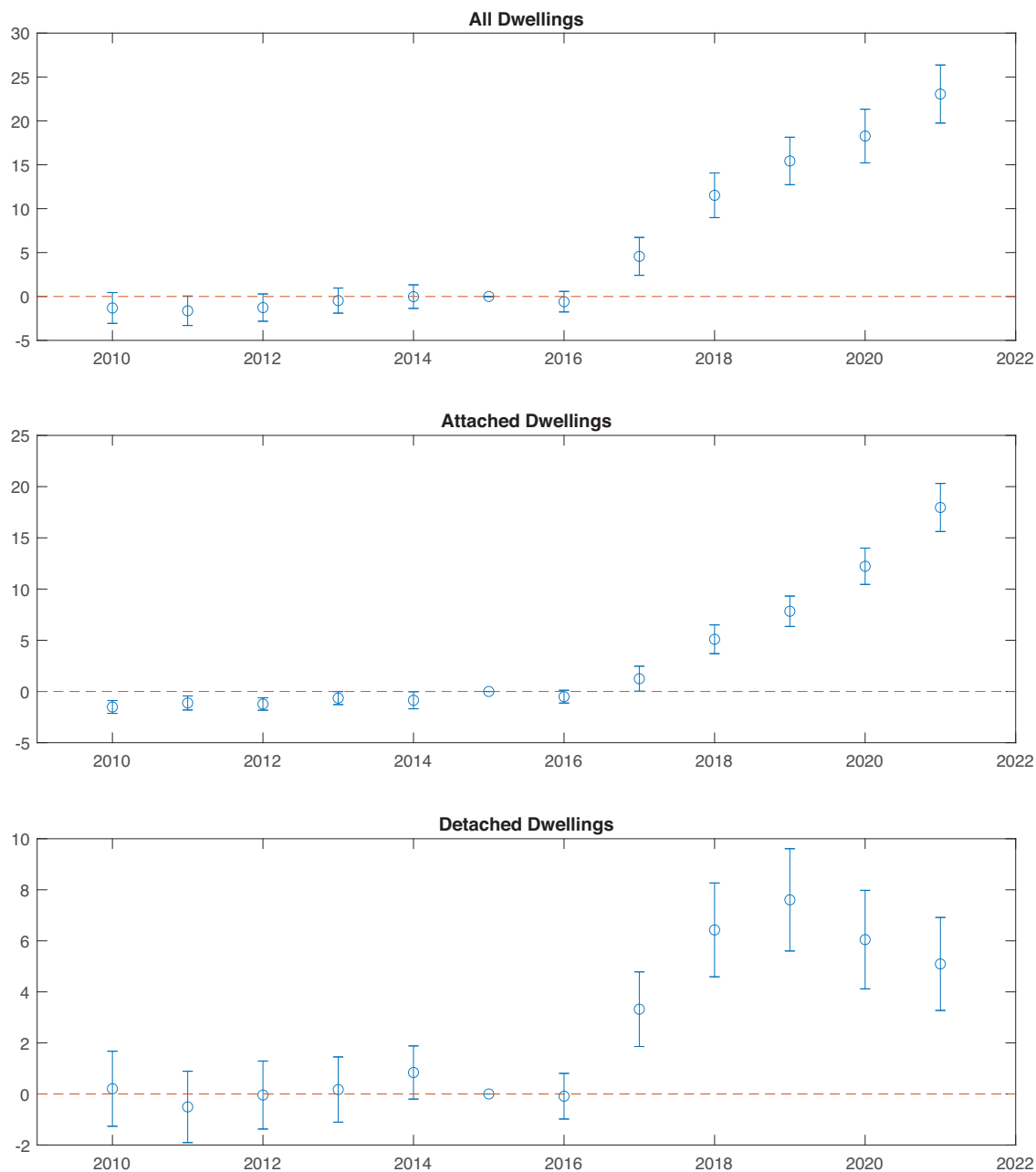


Fig. 20. Estimated Treatment Effects under Specification with Local Period Fixed Effects. *Notes:* Estimated treatment effects (circles) and 95% confidence intervals (error bars). Treatment date is 2015. Outcome is permits per statistical area.

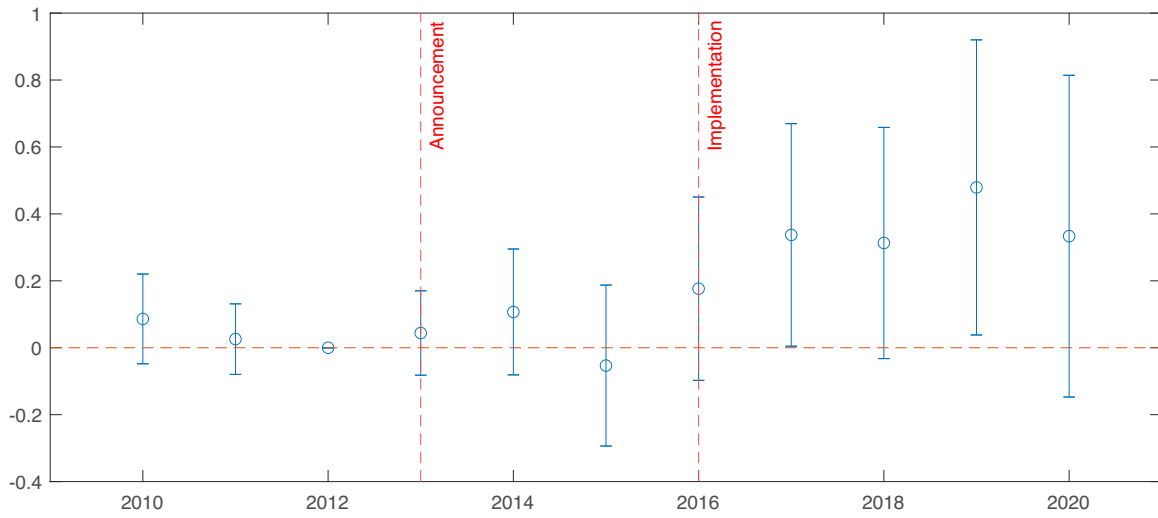


Fig. 21. Estimated Treatment Effects for Housing Sales, 2010-2021. Notes: Estimated treatment effects (circles) and 95% confidence intervals (error bars). Treatment date is 2012. Outcome is log housing sales per statistical area.

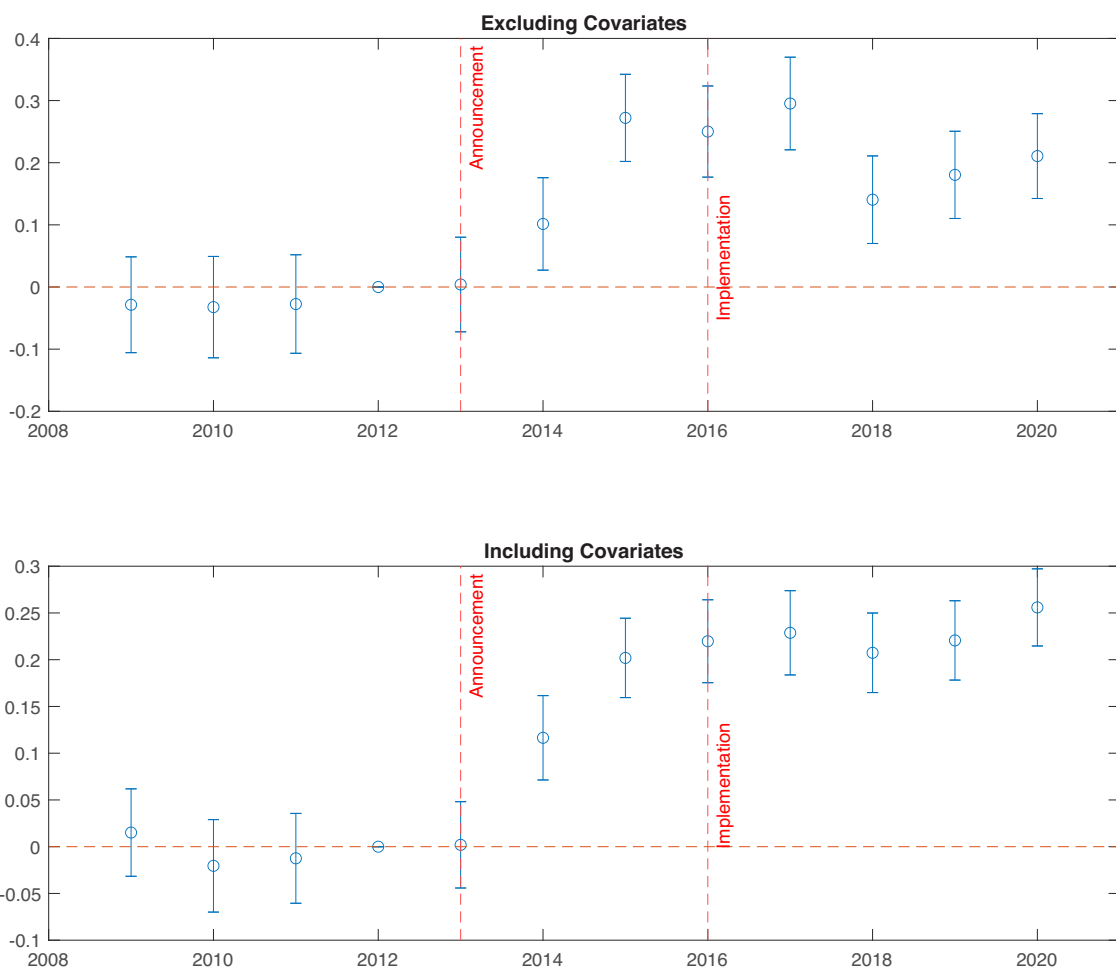


Fig. 22. Estimated Treatment Effects for Land Prices, 2010-2021. Notes: Estimated treatment effects (circles) and 95% confidence intervals (error bars). Treatment date is 2012. Models with (bottom) and without (top) covariate control variables. Outcome is log land prices.

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